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ESSAYS

ON

ASTRONOMY:

A SERIES OF PAPERS
ON PLANETS AND METEORS, THE SUN AND
SUN-SURROUNDING SPACE, STARS AND STAR CLOUDLETS;
AND A DISSERTATION ON THE APPROACHING
TRANSITS OF VENUS.

PRECEDED BY

A SKETCH OF THE LIFE AND WORK OF SIR JOHN HERSCHEL.

BY

RICHARD A. PROCTOR, B.A. CAMB.

Honorary Secretary of the Royal Astronomical Society:

AUTHOR OF 'OTHER WORLDS THAN OURS' 'THE SUN' 'SATURN AND ITS SYSTEM' ETC.

> 'Hither, when all the deep unsounded skies
> Shuddered with silent stars, she clomb,
> And as with optic glasses her keen eyes
> Pierced through the mystic dome,
> Regions of lucid matter taking forms,
> Brushes of fire, hazy gleams,
> Clusters and beds of worlds, and bee-like swarms
> Of suns, and starry streams.
> She saw the snowy poles of moonless Mars,
> That marvellous round of milky light
> Below Orion, and those double stars
> Whereof the one more bright
> Is circled by the other.' TENNYSON.

WITH TEN PLATES AND TWENTY-FOUR DRAWINGS ON WOOD.

LONDON:
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NEW YORK:
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1872.

1855
30

TO

GEORGE BIDDELL AIRY, C.B., LL.D., D.C.L., P.R.S., &c.

ASTRONOMER-ROYAL,

IN RECOGNITION OF THE LONG AND NOBLE SERIES OF LABOURS
BY WHICH HE HAS ADVANCED ASTRONOMY
AND SCIENCE GENERALLY,
AND
IN GRATEFUL RECOLLECTION OF INSTRUCTION RECEIVED FROM
THE MASTERLY TREATISES IN WHICH HE HAS
MADE THE ABSTRUSE CLEAR,

THIS WORK IS RESPECTFULLY DEDICATED
BY
THE AUTHOR.

PREFACE.

I HAVE COLLECTED into the present volume those essays to which I have had most frequently to refer in other works, and particularly in 'The Sun' and 'Other Worlds than Ours.' A certain degree of inconvenience is occasioned to readers when references are made to articles published in different serials, and still more when the reference is to essays published in the 'Proceedings' of scientific societies. It therefore seemed to me desirable to gather together these scattered papers, and, after submitting them to careful revision, to publish them in a single volume. To this course I was further encouraged by the welcome extended to my 'Light Science for Leisure Hours,' in which a series of papers covering a somewhat wider range of subjects had been similarly collected.

The first three essays in the present volume (though the third is entitled, simply, The Study of Astronomy) relate to the life and work of the great astronomer and philosopher whose loss science has recently had to deplore. Then follow papers on the planets Mars and

Saturn. The subject of meteoric astronomy is next treated at considerable length. The recent action of the Astronomical Society in awarding its gold medal to Schiaparelli for his researches into meteoric astronomy has attracted considerable attention to the subject, and has, as it were, sanctioned theories which were viewed somewhat doubtfully when the accompanying essays were written. The papers on the Zodiacal Light and the Solar Corona are chiefly taken from the 'Proceedings of the Astronomical Society.' They present views which have been confirmed, since these papers were written, by many striking discoveries. The remaining essays in the body of the work relate to the stars and star-cloudlets, their nature, movements, arrangement in space, and aggregation into systems. They exhibit the reasoning on which I have based those new views respecting the universe which are presented briefly in 'Other Worlds than Ours,' and which will be more fully exhibited in the lectures I am about to give at the Royal Institution, and in a work I am at present preparing, to be entitled 'Other Suns than Ours.' The Appendices contain notes on the rotation of Mars and the proper motion of the Sun, which seemed somewhat too abstruse for the body of a popular work like the present, as well as three essays on the approaching transit of Venus, which, for a like reason, seemed more suitably placed at the end of the volume than elsewhere. These Appendices, and especially the papers on the transit of Venus, contain many facts which were collected or deduced at the cost of considerable labour, and which will be useful, I believe, to those who desire to give the subjects of these essays a thorough investigation.

The relation of the essay on Equal-surface Projections of the Globe to the more specially astronomical subjects will be obvious when my use of equal-surface projections, as a means of ascertaining and interpreting the laws of stellar and nebular distribution, is taken into account.

As it was found impossible to include in a single volume all the scattered essays which seemed necessary either for purposes of reference, or to supplement the information contained in 'Other Worlds' and 'The Sun,' I separated the essays into two divisions, one including the more strictly scientific essays (with a few exceptions), and the other containing essays of a somewhat lighter kind and more closely associated with the subject of the plurality of worlds. The former series constitutes the present volume, the latter will shortly be published, uniform with 'Other Worlds,' 'The Sun,' and 'Light Science,' under the title of 'The Orbs Around Us.'

I may here point out that popular sketches of scientific subjects, however light they may be in treatment, should be based on a careful investigation of these subjects in their scientific aspect. It will be seen from the dates appended to the present essays that the slighter papers which I have written on the same subjects for our popular serials have been, in all cases, written *after* those subjects had been more solidly dealt with, and that in most instances my researches had been submitted to the consideration of the Astronomical

Society before they had found their way, in a popular form, into our magazines. I point this out in no spirit of egotism. Indeed I conceive that it is simply a matter of duty for those who desire to teach, to prepare themselves for the task by first learning. But I find it desirable to meet a grave charge unjustly brought against me in the 'Saturday Review.' In a pretentious essay on my 'Light Science for Leisure Hours,' the reviewer has implied, in somewhat acrimonious terms, that I have written on subjects which I have not properly studied. He cites, as his chief instance, my views respecting the zodiacal light, remarking that if I had studied the subject, and had made myself acquainted with 'the fact that the zodiacal light maintains a constant position with respect to the horizon,' I should have avoided the blunder of regarding it as an extra-terrestrial phenomenon. It will be seen from the date of the accompanying essay on the subject that I had brought before the Astronomical Society a complete discussion of the phenomena of the zodiacal light full half a year before 'Light Science' was published, while the note at p. 169 will show that in my treatise on Saturn I had announced nearly eight years ago the results of a careful mathematical analysis of the subject. It is true I had not 'made myself acquainted with the fact that the zodiacal light maintains a constant position with respect to the horizon;' but this may be explained by the circumstance that its changes of position, as well from day to day as from hour to hour, are among the most familiar facts of elementary astronomy. Indeed, my critic, throughout his review, appeared singularly anxious to illustrate, by example, the folly of writing on ill-studied subjects.

I have to thank the editors and publishers of the various serials from which these essays have been taken, for the permission to reprint them. In an especial manner I owe thanks to the Council of the Royal Astronomical Society, as well for leave to reprint the essays from the Society's 'Proceedings' (which constitute more than two-fifths of the present volume) as for the permission to use the woodcuts and lithographic engravings belonging to the Society.

RICHARD A. PROCTOR.

BRIGHTON: *April* 1872.

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ASTRONOMICAL ESSAYS.

SIR JOHN HERSCHEL.

On Thursday, May 11, 1871, the greatest astronomer of our day passed from amongst us. In so characterising Sir John Herschel we are not forgetting that others in our time have surpassed him in their mastery of special departments of astronomical science. But, as an astronomer in the true sense of the term, Sir John Herschel stood before all his contemporaries. Nay, he stood almost alone. Others in our day have worked right skilfully and well in advancing astronomy. By abstruse mathematical calculations, by laborious or by most delicate observations, by profound physical researches, or by the ingenious employment of various physical processes, they have added so much to our knowledge that the astronomy of the last generation seems altogether meagre by comparison with that of our own time. But how few have there been who have had, like Herschel, a real insight into the grandeur of astronomical truths! how few who, like him, could so touch the dry bones of fact that they became clothed at once with life and beauty! It may be said of some of the most skilful of Herschel's astronomical contemporaries, that they have scarcely even perceived the essential truths of astronomy; and not many can be truly said to have felt the full import of those truths. But to Herschel astronomy was not a matter of right ascension and declination; of poising, clamping, and reading off; of cataloguing and correcting. He saw the real value of technical and instrumental details; but he did not mistake these details for astronomy, as some have done. When he read the wondrous lessons taught by the heavens, it was for their meaning that he cared, not for the outward symbols by which they are expressed.

Sir John Herschel was born on March 7, 1792, at Slough, in Buckinghamshire. It was here, our readers will remember, that his father's telescopic researches into the celestial depths were carried out, and here the younger Herschel grew up amid influences which could scarcely fail to affect his future career. At Slough night was turned into day, for it was at night and all through the night that Sir William Herschel pursued his labours, while in the daytime the house was kept still and silent, that the astronomer and his assistants might sleep. There must have been something singularly impressive to the mind of young John Herschel in this continued communing with the host of heaven. He saw his father—already an old man when he himself was but a youth,—his uncle and his aunt, Miss Caroline Herschel (the hardworking assistant of her brother), all earnest in the study of those far-off worlds, while the things of this world seemed to be but of secondary import to them. What wonder, then, if as he advanced in years astronomy had for him a significance and a charm which it possessed for none other? Or what wonder, if, when describing the glories of the celestial spaces, he spoke with a fervour and enthusiasm which had a strange power in stirring the hearts of men?

Yet the earlier labours of the future astronomer were directed to other branches of study than the science which his father had made his own. Under the sound instruction of the Scotch mathematician Rogers, he became proficient in mathematics. He went to St. John's College, Cambridge, in 1809, and in 1813 took his Bachelor's degree, graduating as Senior Wrangler, and taking the first Smith's prize. Amongst the most important of his services to science must be noted the work which, in conjunction with a few young men of like mind, he achieved at this time in advancing the study of the higher branches of analytical mathematics. At Cambridge, and indeed throughout this country, the higher mathematics had long been strangely neglected. Continental mathematicians had passed so far in advance of the countrymen of Newton that, as was well said, English mathematicians 'seemed to have slackened rein, conceiving pursuit to be hopeless.' To the labours of Herschel and his fellow-workers—Babbage, Peacock, and others—may fairly be ascribed the success with which, during the last half-century, this state of things has been corrected. A country which can boast of such mathematicians as Cayley, Sylvester, Adams, Airy, Challis, and Stokes, need no longer look with envy even on the highest mathematical schools of France and Germany.

In the very year in which he took his degree Herschel published the treatise entitled 'A Collection of Examples of the Application of the Calculus of Finite Differences.' It is only necessary to examine the papers he contributed at this time to the Royal Society to recognise the mastery he had acquired over the more recondite branches of mathematics. In Volume CIII. of the 'Philosophical Transactions' will be found his first contribution, a paper 'On a Remarkable Application of Cotes's Theorem;' and this was quickly followed up by 'A Consideration of Various Points of Analysis,' by a paper 'On the Development of Exponential Functions and Several New Theorems Relating to Finite Differences,' and by a fine essay 'On Circulating Functions.' Later he studied with success a variety of optical problems, some of them of exceeding difficulty.

Not until the year 1821, or after the practical close of his father's labours in astronomy, did the younger Herschel commence that fine series of researches which constitute his claim to eminence as an astronomical observer. In the years 1821–3, in conjunction with Sir James South, he studied a large number of double stars, and established the fact that many such pairs are physically associated. This fact is now so well known, and so thoroughly admitted, that one reads with wonder of the doubts and objections urged against the existence of physically associated pairs of suns. But the evidence was too strong to be rebutted either by argument or ridicule; and the importance of the work thus achieved by Herschel and South received early recognition from the principal learned societies of Great Britain and the Continent.

In 1825 Herschel began to prepare himself for the researches which he proposed to carry out in surveying the Southern heavens. His object was to obtain, in the first place, 'a sufficient mastery over his instrument'—an excellent Newtonian reflector, 20 ft. long and 18½ in. in aperture. His preparation involved labours which most men would have thought no unworthy occupation for a lifetime. He examined no less than 2,300 nebulæ, of which 525 were discovered by himself. He discovered also, while thus engaged, between three and four thousand double stars. Having thus spent eight years in preparing himself, he left England in November 1833, and reaching the Cape of Good Hope early in the year 1834, he set up in the neighbourhood of Table Bay the instrument with which he proposed to survey the Southern skies. His labours here may be divided into three chief sections (setting aside what may be described as miscellaneous observation):—First, he extended to the Southern heavens his father's system of guaging, making and recording upwards of 2,000 different star-guagings. Secondly, he made a catalogue of 1,700 Southern nebulæ. Thirdly, he catalogued more than 2,000 Southern double stars. For four years and a quarter he remained at the Cape, returning to England in 1838. Nine years passed, however, before the results of his labours were fully published in that most valuable and masterly treatise entitled 'Results of Observations made during the years 1834–38 at the Cape of Good Hope; being the Completion of a Telescopic Survey of the Whole Surface of the Visible Heavens, commenced in 1825.' This work does not include, however, an account either of his meteorological researches at the Cape or of his labours in perfecting the system of national education in the Cape colonies.

In 1836 the Astronomical Society gave Herschel their Gold Medal. A year later he was made a baronet, an event which many of his scientific admirers contemplated with little satisfaction. More to the purpose was the proposal made in 1839 that he should succeed the Duke of Sussex in the Presidential Chair of the Royal Society. This proposal, however, he declined.

Sir John Herschel's subsequent labours were devoted rather to scientific instruction than to original researches. He had already written more than one work on science—a 'Treatise on Sound,' another on the theory of light, his 'Preliminary Discourse on the Study of Natural Philosophy,' and (in 1830) his 'Treatise on Astronomy.' In 1849 he published his well-known 'Outlines of Astronomy,' enlarged from the last-named work. Of other works published separately, we need only mention his excellent treatises on meteorology and physical geography. But he wrote, also, many articles in the 'Edinburgh Encyclopædia' and the

'Encyclopædia Britannica,' as also in the *Edinburgh Review*, the *Quarterly Review*, and other serials. Some of his shorter papers have been reprinted in a work entitled 'Familiar Essays on Scientific Subjects.'

As a theoriser in astronomy, Herschel was not equal to his father. There is nothing in his works comparable with the grand progression of his father's ideas respecting the structure of the universe. He had, indeed, no great power of grasping facts, insomuch that we are over and over again surprised by his recurrence to theories which he has himself shown to be negatived by observed relations. The source of his father's success in mastering the secrets of the universe lay in his power of retaining in his thoughts all known facts bearing upon the subject he was dealing with. Hence that steady progression from truth to truth, or rather from the less complete to the fuller recognition of truth; insomuch that William Struve, speaking of the latest of Sir William Herschel's papers, said justly:—'Heureux mortel que fut Herschel, de jouer, à l'âge de 80 ans, d'une pénétration de l'esprit et d'une clarté du jugement qui le firent composer ses deux derniers mémoires, remplis d'une spéculation sublime et profonde!' But if the great astronomer who has lately departed from amongst us was inferior to his father in this respect, so also have been all others; while Sir John Herschel alone, of all who have succeeded the elder Herschel, has been fairly comparable with him in all respects save this.

As regards Sir John Herschel's qualities as a populariser of science, we venture to express an opinion somewhat at variance with that commonly entertained (we believe) upon the subject. That he was a successful populariser is undoubted, but the mode and reason of his success have been we think, misunderstood. The literary merits of his writings are certainly not exceptionally great. His style is often ponderous, and not unfrequently far from clear. Nor can it be said that he has successfully expounded the difficulties dealt with in his treatises. The buyers of his 'Outlines,' have been, we believe, many times more numerous than the readers of that work; nor is this greatly to be regretted when it is remembered how much there is in the work which could be of no use to nine hundred and ninety-nine out of every thousand students of astronomy.

Where, then, was the secret of Herschel's success—for successful he undoubtedly was—in attracting to the study of astronomy hundreds who but for him would have cared little for that science? There can be no question, we believe, that the answer must be sought in the considerations touched upon in the beginning of this paper. His soul was so thoroughly imbued with the sense of the sublimity of the lessons taught by the celestial depths, that his descriptions, despite all faults of style, are irresistibly impressive. Here by a word, there by a happy turn of expression, now by some strikingly poetical conception, anon by a grand array of noble thoughts, he forces his readers to share his own enthusiasm. There are some passages in his writings which for grandeur and sublimity are surpassed by nothing that has been written in the English language, save, perhaps, some few portions of the 'Paradise Lost.'

English Mechanic for May 19, 1871.

SIR JOHN HERSCHEL AS A THEORIST IN ASTRONOMY.

It would be difficult to say in what department of astronomical research Sir John Herschel was most eminent. That he was the greatest astronomer of his day, even those who rivalled or surpassed him in special departments admit without question. He was, indeed, *facile princeps* not merely among the astronomers of his own country, but among all his astronomical contemporaries. He held this position chiefly by reason of the wide range of subjects over which his mastery extended. He was unequalled, or rather unapproached, in his general knowledge of the science of astronomy. It need hardly be said that he was proficient in the mathematical departments of the science. (Perhaps no one of whom this cannot be said may be regarded as an astronomer at all.) In his knowledge of the details of observatory work he was surpassed by few, and his acquaintance with the specialities of astronomical instruments was such as might have been anticipated from the excellence of his mathematical training. He was far the greatest astronomical observer the world has known, with one single exception—Sir W. Herschel. That in certain respects other observers surpassed him may be admitted very readily. He had not the eagle vision of Dawes or Goldschmidt, for instance; nor had he the aptitude for accurately measuring celestial spaces, angles, and so on, which some of the German astronomers have displayed of late years. But such *minutiæ* as these may well be overlooked when we consider what Sir J.

Herschel actually achieved as an observer. Thousands of double stars detected, measured, and watched as they circled round each other; upwards of two thousand nebulæ discovered; the southern heavens guaged with a twenty-feet telescope—these, and like achievements, dwarf into insignificance all the observational work accomplished by any single astronomer since Sir W. Herschel ceased his labours. In one respect, and that noteworthy, Sir John Herschel even surpassed his father. Only one astronomer has yet lived who had surveyed with a powerful telescope the whole sphere of the heavens—that astronomer was the younger Herschel. He went over the whole range of his father's observations, in order (to use his own words) that he might obtain a mastery over his instrument: then in the southern hemisphere he completed the survey of the heavens. He alone, then, of all the astronomers the world has known, could boast that no part of the celestial depths had escaped his scrutiny. I need not dwell on Sir John Herschel's success in expounding the truths of astronomy. We owe to him, beyond all question, the wide interest at present felt for the science, as well as the special fervour with which the younger astronomers of our day discuss its truths. And, lastly (passing over many departments of astronomical study), Sir John Herschel's position as a theorist in astronomy is unquestionably a most eminent one. My present purpose is to discuss his work in this direction; to endeavour to exhibit the special merits of his mode of theorizing; and, if it should happen that in my judgment certain features of Herschel's work in this direction should seem less excellent than the rest, to exhibit the ground on which such judgment is based —truthfully, as is right, but also with fit consideration of the respect (perhaps I should rather say the reverence) due to the memory of the greatest and the most amiable philosopher of our times.

In the first place, let the position of scientific theorizing be rightly apprehended. We hear much of theory and practice, or, in the case of such a science as astronomy, of theory and observation, as if the two were in some sense opposed to each other. Nay, unfortunately, it is not uncommon to hear some observers speak of the astronomical theorist as if he held a position quite apart from theirs. Theorists do not, on the other hand, adopt a corresponding tone in speaking of observers. And this for a very simple reason—the theorist must needs value the labours of the observer, because it is on such labours that he must base his theories. But observers—at least such observers as do not themselves care to theorize—are apt to contemn the theorist, to suppose that the hypotheses he deals with have been evolved from the depths of his moral consciousness, instead of being based on those very observations which they mistakenly imagine that the theorist undervalues. The fact, indeed, is really this—that the theorist alone values observation as fully as it deserves. The observer is too apt to value observations for their own sake; the theorist sees in them a value beyond that which they possess in themselves—a value depending on their relation to other observations, as well as a value depending on the application of suitable processes of manipulation, or, as it were, of manufacture. It is not going too far, indeed, to say that observations as originally made are as raw material—highly valuable it may well be (and the manufacturer will be better aware of this than the producer of the raw material), but owing their value to their capacity for being wrought into such and such fabrics. It would be as reasonable for the miner to despise the smith and the engineer, as for the observer in science to contemn him who interprets observations and educes their true value.

Let me quote here a passage from those too little studied essays, the papers contributed by Sir W. Herschel to the 'Transactions of the Royal Society.' The passage is interesting as belonging to the opening of that noble essay in which he first presented to the world his ideas respecting the constitution of the celestial depths. 'First let me mention,' he says, 'that if we would hope to make any progress in investigations of a delicate nature, we ought to avoid two opposite extremes, of which I can hardly say which is the most dangerous. If we indulge a fanciful imagination and build worlds of our own, we must not wonder at our going wide from the path of truth and nature; but these will vanish like the Cartesian vortices, that soon gave way when better theories were offered. On the other hand, if we add observation to observation, without attempting to draw not only certain conclusions but also conjectural views from them, we offend against the very end for which only observations ought to be made.' 'I will endeavour,' he adds, speaking of the special work he was then engaged upon, 'to keep a proper medium; but if I should deviate from that, I could wish not to fall into the latter error.'

The power of forming sound theories depends on many mental qualities and habitudes—some positive, some negative. I propose to consider the chief of these, in about the order in which they are called into exercise in the gradual progression whereby a theory advances to its final stage, —illustrating each by the work of the great astronomer whose position as a theorist is my present theme.

Sir John Herschel has himself described in clear and powerful language the quality which is primarily requisite in the theorist. 'As a first preparation he must loosen his hold on all crude and hastily-adopted notions, and must strengthen himself by something like an effort and a resolve for the unprejudiced admission of any conclusion which shall appear to be supported by careful observation and logical argument, even should it prove of a nature adverse to notions he may have previously formed for himself, or taken up, without examination, on the credit of others. Such an effort is, in fact, a commencement of that intellectual discipline which forms one of the most important ends of all science. It is the first movement of approach towards that state of mental purity which alone can fit us for a full and steady perception of moral beauty as well as physical adaptation. It is the "euphrasy and rue" with which we must "purge our sight" before we can receive and contemplate as they are the lineaments of truth and nature.'

These just principles have been perhaps as clearly laid down by other men of science; but it may be questioned whether any has ever more thoroughly obeyed them than Sir John Herschel. The enforced mental purity with which he approached a subject on which he proposed to theorise was indeed so remarkable that to many it was scarce even intelligible. His determination to remove from his own mind all the effects of preconceived opinions, whether adopted independently or received at the hands of others, was mistaken by some for an undue humility of mind. Nay, one biographer went so far as to ascribe to a spirit of flattery (and that spirit the offspring of vanity! *) that characteristic which, rightly understood, marked Sir John Herschel's mind as subservient to truth alone.

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* The obituary notice in which this remark appeared was obviously written by an able man, and one who held in very high respect the abilities of Sir John Herschel; and notwithstanding the feeling of pain with which I conceive every admirer of Sir John Herschel must have read the passage, I imagine that no one was disposed to question the writer's honesty of purpose. Professor Tyndall, in a feelingly-written letter, challenged the writer of the passage to make known his name and to defend his opinion. From internal evidence in the obituary notice itself, I am disposed to believe that, apart from the reasons assigned by the editor for the non-acceptance of this challenge, there was one very excellent reason why the writer could not respond to a challenge which would have been to him as the trumpet to the war-horse not very long ago. Unless I am deceived, the author of the biography did not live to see it in print.

The completest proof which a man of science can give of this 'mental purity' is afforded by a readiness to submit to some crucial test a theory which he has strong reasons for desiring to see established. I draw a distinction here between testing a theory and the search for evidence respecting a theory. One who is not free from prejudice may yet none the less eagerly search for evidence respecting the

It would be idle to defend Sir John Herschel from the charge of vanity—a charge which could only have had birth in a total misapprehension of the singular sweetness of disposition which endeared the great astronomer not only to all who knew him personally, but to many (the present writer among the number) who, without being personally acquainted with him, received from him written words of encouragement and kindness. Yet it may be permitted me to point out (earnestly disclaiming, the whilst, all notion that the argument is needed in Sir John Herschel's defence) the utter fallacy of the reasoning by which the charge of vanity was supported. It is unquestionably true that flattery is always the offspring of vanity or of a worse failing; and if compliments addressed to others on the score of their views or theories be admitted to be untrue, the charge of flattery is established, and with it the charge of vanity of disposition. But when such compliments relate to opinions opposed to those held by the person who pays them (and it was the very basis and main support of the attack on Sir John Herschel that this was the case), the argument against vanity is at once seen to be altogether stronger than the argument in its favour founded on the suspicion of flattery. For a vain man may well be supposed to flatter others in matters not affecting his own vanity, in order that he in turn may be flattered in those matters respecting which he is vain. But the spirit of detraction itself could not force any man to believe that a vain person would, for the sake of praise, overpraise another to his own dispraise. A systematic readiness to give to others their due, even though at his own cost, must surely be explained as arising from a genuine desire to do justice. Such a desire may be, unfortunately, far less common than could be wished; but the unusual nature of a form of excellence is no valid reason for preferring some utterly incongruous evil motive in explanation of conduct obviously suggesting such exceptional excellence of disposition.

No one who had occasion to seek the opinion or advice of Sir John Herschel could fail to be struck by his exceeding courtesy, and by the readiness with which he admitted or noted errors into which he might have fallen (as all men will). And yet I think that those who possess letters written by him, and will carefully examine them, will find, for each error admitted by him, at least two pointed out in their own views. Indeed, any one who objected to be set right when in error might well be disposed to regard Sir John Herschel as a merciless correspondent, notwithstanding the calm courtesy of his remarks. He set truth in the first place; and by comparison with her, neither his own opinions nor those of others were permitted to have any weight whatever.

theories he desires to advocate. But to test a theory crucially, to enter on a series of researches which must needs reveal the weak points of a theory, this is what only the true man of science is capable of. 'This,' as Professor Tyndall well remarks, 'is the normal action of the scientific mind. If it were otherwise—if scientific men were not accustomed to demand verification—if they were satisfied with the imperfect while the perfect is attainable, their science, instead of being, as it is, a fortress of adamant, would be a house of clay, ill fitted to bear the buffetings of the storms to which it has been from time to time, and is at present, exposed.'

Now, when Sir John Herschel commenced his labours as an astronomer, there were two theories before the world, respecting which it may fairly be asserted that had he regarded them with a feeling amounting to strong prejudice in their favour, he might have claimed forgiveness. They were of unequal importance, but each was full of interest.

The first related to those double stars which now form so favourite a subject of study with the amateur astronomer. His father, commencing the investigation of these objects under the impression that the two stars which seemed to form each pair were but accidentally seen nearly in the same direction, had been led after long labours to the conclusion that the double stars are for the most part real star-couples, physically associated by the mighty bond of their common attraction. A strange theory in those days, though now so commonly admitted—a theory not yet established by the evidence which had been adduced in its favour at the time when Sir John Herschel's career as an observer commenced. The theory admitted of a ready test at that time, however; for Sir William Herschel had recorded more than thirty years before the aspect of many hundreds of these objects, and it required only that all the double stars thus pictured by the elder Herschel should be submitted to a new and searching scrutiny, in order to set at rest at once and for ever the question whether they were physically associated. If they were, some among them must needs be circling round each other at a rate rendering their motions recognisable. It needed only that these should be selected from the rest by a comparison with Sir William Herschel's researches, and then watched as they moved around their common centre, in order to prove that double-sun systems, wonderful as the idea might seem, have yet a real existence. On the other hand, the test was a crucial one. If no such signs of motion as the elder Herschel had suspected were found in reality to exist, it would be proved that that great astronomer had been mistaken in the theory itself, which had seemed so full of interest.

The younger Herschel, entering into alliance with James South, submitted his father's theory respecting the double stars to this most thorough test—with a result which is known to all students of astronomy. Plain proof was obtained that many double stars are physically associated, and thus the strange theory of coupled suns was placed on a firm basis.

The second theory above referred to was far more important. Sir William Herschel's long survey of the northern skies had led him to form and to enunciate those grand views respecting the constitution of the heavens with which his name will for ever remain associated. I do not propose here to discuss the principles of research adopted by Sir William Herschel, either in his star-guaging or in the survey of the celestial cloudlets which astronomers call nebulæ. Nor shall I here inquire into the reasoning by which he was led to those noble generalisations which constituted his theory respecting the construction of the universe. What I principally desire to do in this place is to show with what readiness Sir John Herschel subjected theories which he undoubtedly held in the highest respect to the most severe test to which they could by any possibility be exposed.

Of the reverence with which the younger Herschel regarded the noble labours and the grand conceptions of his father it is perhaps needless to speak. He has, indeed, been blamed, by those who misunderstood his disposition, for carrying that reverence to excess, insomuch that one writer has not scrupled to speak of the manner in which Sir John Herschel regarded the instruments his father had employed as approaching in its nature to idolatry.* Altogether denying the justice of such views as these, we must yet recognise the fact that if any theories could have so far found favour in Herschel's sight as to cause him to forget the rules which he had laid down for his own guidance, and to seek rather for evidence confirming those theories than for experiments by which their value might be tested, it would have been to his father's theories respecting the constitution of the universe that he would have been disposed to extend this indulgence.

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* In the biographical notice to which I have referred above, the statement is made that Sir John Herschel had 'so specially sanctified his idol' (his father's forty-feet reflector) 'that he could not cheerfully bear to hear it lightly spoken of;' and elsewhere in the same notice, that in speaking of this instrument he 'altogether left an impression that a little less sensibility and a little more sense would have saved a good deal of mortification.' 'These be very bitter words;' and if it chanced that they were true, we might yet regard their utterance as in exceedingly bad taste—first, because they are personal, and secondly, because they bear no relation to those parts of Sir John Herschel's life which may be regarded as of public interest. But I venture to express the conviction that those who will carefully study Sir John Herschel's remarks respecting his father's largest telescope will not adopt his biographer's interpretation of those remarks. I have further the means of showing that Sir John Herschel's views respecting this instrument were not such as have been here ascribed to him. I may be permitted to quote from a letter addressed to myself upon the subject, partly because of Sir John Herschel's repeatedly-expressed willingness to permit remarks in his letters to be quoted, and partly because the publication of his own words in this special instance may serve to remove a false and unjust impression respecting his disposition. As it chances that the opinion expressed in the passage I am about to quote is directly opposed to one I had myself publicly expressed, I find a further reason for desiring to make the passage known. I had asked him whether he thought (as I mentioned that I had) that his father had really discovered four additional satellites of the planet Uranus. 'As to these four satellites,' ran his reply (which lies before me as I write), 'I incline to the opinion that my father must have too readily persuaded himself that the minute points of light which from time to time *he undoubtedly saw*, were *all really* satellites. The testimony of Lord Rosse's and Mr. Lassell's reflectors—which are composed of metal much more reflective than even that of the eighteen-inch, and *very* much more than that of the four-feet reflector of my father—I think must be held conclusive.' (The italics are his.)

Yet the noblest series of observations made by the younger Herschel was so devised as to afford a crucial test of the accuracy of his father's views respecting the constitution of the heavens. The elder Herschel had shown that certain relations prevail among the celestial objects visible at his northern observatory, and it was on the existence of those relations that his theories were founded. It is clear, however, that the mere accident that the observation of the celestial sphere had been first prosecuted in northern latitudes ought not to affect the views which men should form respecting the heavens. The terms North and South have relation to this little earth on which we live, *not* (properly speaking) to the celestial sphere, though they have become in a sense associated with that sphere. We speak of the North Pole of the heavens and of the South Pole of the heavens, and again of the revolution of the celestial sphere, because the rotation of our own earth seems to give a reality to these conceptions. But in judging of the constitution of the heavens we are bound to lay aside this usage, or at least to remember that it bears no real relation to the system of stars. We are placed in the midst of this vast system as a traveller in the midst of some vast forest, and the configuration of the system is no more associated in reality with the position in which our earth's axis chances to be situated, than the shape of a forest is associated with the direction in which the traveller pleases to pursue his course.

Sir William Herschel, then, had studied the northern heavens much as a traveller might study the aspect of those parts of a forest towards which his course was leading him. The southern heavens, or those parts of them which are never seen in our latitudes, were quite as well able to supply information respecting the constitution of the sidereal system as those which Sir William Herschel had surveyed. And it is clear that if the elder Herschel had rightly interpreted the northern skies, the southern skies should teach precisely the same lesson: whereas, if in his speculations concerning the northern heavens he had mistaken accidental peculiarities for essential features of the celestial spaces themselves, then the study of the southern heavens could scarcely fail to reveal his mistake and (probably) to explain its source.

To this arduous task—a task which, even if its results were favourable, would add little to the admiration with which his father's work was contemplated by all who understood its purport; while, if unfavourable, it would serve to negative all his father's hypotheses—Sir John Herschel devoted twenty-one years of his life. Eight years he passed in preparation, that preparation consisting in the complete re-survey of the northern skies; four years at the Cape of Good Hope, in the survey of the southern heavens; and lastly nine years in reducing his observations to form and presenting them in his own effective manner, in one of the most masterly scientific treatises the world has yet seen. In the presence of such noble labours, conducted in a spirit so philosophic, the fact that the theories of the elder Herschel were in all their more important features most amply confirmed, seems to sink almost into insignificance. We feel that, loving as was the reverence with which Sir John Herschel contemplated his father's work, he had set scientific truth far above that reverence. He had entered cheerfully on labours which might have resulted in shaking men's faith in his father's opinions; and no question can exist that, had this been the result, it would have been as fully exhibited to the world as that which actually rewarded Sir John Herschel's researches.

The next quality which is called into action in the formation of theories is the power of seeing the full meaning of observed facts—of seeing beneath the surface, so to speak—since observed facts often, on the face of them, show little which tends to enlighten the inquirer. In order to explain my meaning, I will take two instances from the history of observations made upon the planet Saturn. When Galileo first turned his telescope upon this planet he imagined that he could see on either side of a central disc two other discs, each nearly half as large as the central one. He watched the planet on several nights, seeing always this appearance. But when at a later season he viewed the planet, the two side discs had vanished. They reappeared again after a time; and, as he continued to watch the planet, he saw them change somewhat in size and shape, but they always remained at an unchanged distance from the central disc. Now it can be demonstrated that, by means of abstract reasoning alone, quite independently of that increase of optical power which subsequently enabled Huyghens to interpret these appearances, Galileo might have convinced himself that Saturn is girdled about by a flat ring inclined to the path in which the planet travels. Here was an instance, then, where an observed fact implied in reality much more than it seemed to do at first sight. The other instance is of like nature. The observer Bond (the elder), of America, noticed on the brightest of the rings of Saturn two shaded regions, symmetrically placed, close by the inner boundary of this ring, and at the two ends of the oval into which this inner outline is foreshortened. (See Plate I.) The observation in itself seems to be rather perplexing than instructive; but it is the perplexing observations which, in the long-run, best repay careful study, for they can usually be only explained in one way. I have been able to show that this particular observation (if admitted) proves beyond all possibility of question that where these shaded regions appear we see, *through the ring*, the dark sky beyond.*

I know of no more remarkable instance of Sir John Herschel's readiness and skill in interpreting observed facts than the way in which he dealt with the features he had recognised in the Magellanic Clouds. He was the first to survey those strange celestial regions with a powerful telescope. He mapped down and pictured multitudes of star-cloudlets, scattered among the myriads of minute stars which produce the milky light of the Magellanic Clouds. At this point others might have ceased their labours. *There* was an array of interesting objects within a certain region of the heavens —what more could be said? But Sir John Herschel was not thus satisfied. He reasoned from the shape of the Magellanic Clouds to the distances of the star-cloudlets within them, and thence to the scale on which these star-cloudlets are formed. He was able to deduce in this way perhaps the most important conclusion to which astronomers have ever been led by abstract reasonings—a conclusion interpreted by Whewell, Herbert Spencer, and in my own inquiries into the star-depths, to mean nothing short of this: that, so far as the only available evidence we have is concerned, all orders of star-cloudlets belong to our own star system, and not to external galaxies.

For another instance of Sir John Herschel's power in this respect, I would refer the reader to his discussion of the phenomena presented by Halley's comet during its approach towards and recession from the sun in the years 1835–1836. A brief *résumé* of this discussion will be found in the charming volume entitled 'Familiar Essays on Scientific Subjects;' but the student of astronomy should also read the original paper in the 'Results of Astronomical Observations made at the Cape of Good Hope.' Here I shall merely quote the conclusion of the reasoning, as summarised in the 'Familiar Essays,' in order to show how much which was certainly not directly contained in the observations was deduced in this instance by abstract reasoning. It was 'made clear' that the tail of this comet 'was neither more nor less than an accumulation of luminous vapour, darted off, in the first instance, *towards* the sun, as if it were something raised up, and as it were exploded, by the sun's heat, out of the kernel, and then immediately and forcibly turned back and repelled *from* the sun.'

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* 'Saturn and its System,' pp. 118–121. The reasoning in these pages is not hypothetical, but demonstrative; though of course the demonstration fails if the observed relation should be shown to have no real existence. There are other reasons for believing that we can see through the Saturnian rings, and that these are formed of disconnected satellites; but the evidence given by these shaded regions is singularly simple and effective.

Another faculty which the theorist should possess in a high degree is a certain liveliness of imagination, whereby analogies may be traced between the relations of the subject on which he is theorising and those of objects not obviously associated with that subject. This faculty Sir John Herschel possessed in a very high degree—almost as strikingly as his father, who in this respect probably surpassed all other astronomers, unless we place Kepler and Newton on the same level. It is obvious that the faculty is of extreme importance, though it is one which requires a judicious control, since if it be too readily indulged it may at times lead us astray.

One of the finest illustrations of Sir John Herschel's aptitude in tracing such analogies is to be found in his reasoning respecting the zones in which the solar spots ordinarily make their appearance. I give this reasoning as it was originally presented in the fine work to which I have already so often referred, the 'Results of Observations made at the Cape of Good Hope.' 'Whatever be the physical cause of the spots,' says Herschel, 'one thing is certain, that they have an intimate connection with the rotation of the sun upon its axis. The absence of spots in the polar regions of the sun, and their confinement to two zones extending to about latitude 35 degrees on either side, with an equatorial zone much more rarely visited by spots, is a fact which at once refers their cause to fluid circulations, modified, if not produced, by that rotation, by reasoning of the very same kind whereby we connect our own system of trade and anti-trade winds with the earth's rotation. Having given any exciting cause for the circulation of atmospheric fluids from the poles to the equator and back again, or *vice versâ*, the effect of rotation will necessarily be to modify those currents as our trade winds and monsoons are modified, and to dispose all those * meteorological phenomena on a great scale, which accompany them as their visible manifestations, in zones parallel to the equator, with a calm equatorial zone interposed.' Herschel then proceeds to inquire 'what cause of circulation can be found in the economy of the sun, so far as we know and can understand it.' With this inquiry, however, we are not at present concerned, save only to note how the aptitude of the theorist in the recognition of analogies leads him to inquiries which otherwise he would not have entered upon.

Sir John Herschel, indeed, entertained a singularly strong belief in the existence of analogies throughout the whole range of created matter. As an evidence of this I venture to quote a passage from a letter of great interest, which I received from him in August 1869. It relates to the constitution of the heavens, referring especially to a remark of mine to the effect that all forms of star-clouds and star-clusters seem to be included within the limits of our own sidereal system. 'An opinion,' he wrote, 'which the structure of the Magellanic Clouds has often suggested to me, has been strongly recalled by what you say of the inclusion of every variety of nebulous or clustering form within the galaxy—viz., that if such be the case, that is, if these forms belong to and form part and parcel of the galactic system, then *that system includes within itself miniatures of itself* on an almost infinitely reduced scale; and what evidence then have we that there exists a universe beyond? —unless a sort of argument from analogy that the galaxy, with all its contents, may be *but one* of these miniatures of that vast universe, and so on *ad infinitum;* and that in *that* universe there may exist multitudes of other systems on a scale as vast as *our* galaxy, the analogues of those other nebulous and clustering forms which are *not* miniatures of our galaxy.'

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* In the text the word is *their*. I think the word must have been written *those*.

This, perhaps, is the grandest picture of the universe that has ever been conceived by man.

Next in order comes that faculty by which the chain of causes and effects (or of what we call such) is traced out, until the true correlation of all the facts dealt with by the theorist is clearly recognised. Adequately to illustrate the action of this faculty, however, would obviously require more space than is available in such a paper as the present. I shall mention but one instance of Sir John Herschel's skill in this respect, selecting for the purpose a passage (in the first edition—1833—of his treatise on astronomy), the opinions expressed in which have been erroneously supposed to have been in the first instance enunciated by the celebrated engineer, George Stephenson. Tracing out the connection between the action of the central luminary of our system and terrestrial phenomena, Sir John Herschel remarks that 'the sun's rays are the ultimate source of almost every motion which takes place on the surface of the earth. By its heat are produced all winds, and those disturbances in the electric equilibrium of the atmosphere which give rise to the phenomena of lightning, and probably also to those of terrestrial magnetism and the aurora. By their vivifying action vegetables are enabled to draw support from inorganic matter, and become in their turn the support of animals and of man, and the sources of those great deposits of dynamical efficiency which are laid up for human use in our coal strata. By them the waters of the sea are made to circulate in vapour through the air, and irrigate the land, producing springs and rivers. By them are produced all disturbances of the chemical equilibrium of the elements of nature, which by a series of compositions and decompositions give rise to new products and originate a transfer of materials. Even the slow degradation of the solid constituents of the surface, in which its chief geological changes consist, is almost entirely due, on the one hand, to the abrasion of wind and rain and the alternation of heat and frost, and, on the other, to the continual beating of the sea-waves, agitated by winds, the results of solar radiation.' He goes on to show how even 'the power of subterranean fires,' repressed or relieved by causes depending on the sun's action, 'may break forth in points where the resistance is barely adequate to their retention, and thus bring the phenomena of even volcanic activity under the general law of solar influence.'

As respects Sir John Herschel's skill in devising methods for throwing new light on questions of interest, it is only necessary to remark that we owe to him the first experimental determination of the quantity of heat received from the sun, as well as a solution of difficulties which seemed to

Sir William Herschel almost insuperable in the problem of estimating the relative brightness of the lucid stars. I may add also that he was among the first, if not actually the first, to suggest that the prismatic analysis of solar light might 'lead us to a clearer insight into its origin.'

Nor is it necessary to dwell specially on that most notable quality of Sir John Herschel's character as a theorizer—the light grasp with which he held those theories which he had himself propounded. This characteristic is so intimately associated with the mental purity the necessity for which Sir John Herschel kept so constantly in his mind (as I have shown above) that, having exhibited instances of the last-named quality, it is hardly necessary to point to cases by which the other has been illustrated. Suffice it to say that no theorist of modern times has surpassed Herschel, and few have equalled him, in that complete mastery of self whereby it becomes possible for the student of science not merely to admit that he has enunciated erroneous opinions, but to take in hand the theories of others, and to work as patiently and skilfully in placing such theories on a firm basis as though they had been advocated in the first instance by himself. I know no more perfect proof of strength than this lightness of hold, especially in the case of theories which may for many years have been among the favourite views of the theorizer. To those who have never theorized, it may seem the easiest thing in the world to abandon a long-favoured theory. How difficult it really is, however, is shown by the persistence with which even eminent students of science have struggled to maintain their theories long after the most convincing evidence has been obtained against them. Unfortunately for science, the lightness of grasp with which the Herschels, father and son, held their most favoured theories is even more uncommon than the observing skill, the untiring patience, and the ingenuity of device with which they sought for evidence to establish the truths of astronomy.

One quality alone Sir John Herschel seems to me (I venture the opinion with extreme diffidence) to have possessed in a less eminent degree than those other qualities which are necessary for successful theorizing. Lightness of grasp for theories needs to be accompanied by a most rigid grasp of facts. I conceive that in some instances Sir John Herschel held facts almost as lightly as he held theories. Let me not be misunderstood. I would by no means desire to imply that Sir John Herschel in any instance wittingly overlooked known facts. To suppose, indeed, that this was my meaning would be to suppose that at the close of this paper I desired to present Sir John Herschel to the reader in quite a different light than in the earlier paragraphs. I would merely note that in some instances Sir John Herschel seemed to forget that certain facts had already been established—even sometimes that he had himself established such and such facts. It is, of course, always possible that where I thus suppose him to have been forgetful of facts which he had either already admitted or established, I have in reality misunderstood either his opinion of the facts or those statements of his which seem to me at variance with such facts. And yet—to take an instance which is more particularly in my thoughts at this moment—I have not been alone in interpreting Sir John Herschel's own remarks about the Magellanic Clouds to imply that, in the only instance in which any determination of the distances of the several orders of nebulæ has been possible, nebulæ of *all* orders have been found to lie far within the limits of distance to which our own star system extends. As I have already mentioned, Dr. Whewell and Mr. Herbert Spencer took precisely the same view of Sir John Herschel's reasoning that I have taken; and, indeed, for my own part, I can conceive no other interpretation, either of his reasoning, or of the facts on which his reasoning was based. Yet I think that I am not mistaken in believing that much which has since been written by Sir John Herschel about the nebulæ is wholly at variance with the 'demonstrated fact' of the remarkable sentence in which he summed up his reasoning about the Nubeculæ. This, at any rate, is certain, that the views which Dr. Whewell, Mr. Herbert Spencer, and I myself have expressed about the nebulæ (views identical so far as they overlap) have been commonly regarded as differing from the opinions entertained by Sir John Herschel respecting nebulæ long after he had enunciated the 'demonstrated fact' referred to above.*

Other instances might be cited, which seem almost as decisive of the fact, that in this special respect Sir John Herschel was not equal to his father, the solidity of whose reasoning was never in a single instance marred by a forgotten fact. It may, indeed, be regarded as in no sense wonderful if one whose labours extended over so enormous—one may even say, without forgetting his father's work, so unparalleled—a range as Sir John Herschel's, forgot sometimes those facts which he had already admitted on the evidence obtained by others, or even those which he had himself established.†

But even if this blemish have a real existence, it is but as a spot upon the sun. It bears no further than *this* upon our opinion of Sir John Herschel's position as a theorist in astronomy: that whereas but for this occasional forgetfulness he might have ranked higher than Sir William Herschel himself, we must now concede that the younger Herschel was second to the elder, but to the elder Herschel alone. A remarkable era in astronomy, observational and theoretical, has come to a close with the death of Sir John Herschel—an era lasting nearly a full century, during which two astronomers, father and son, have stood forth more prominently than any save the very greatest in astronomical history. With all our faith in the progress of the human race (and my own faith in that progress is very strong), we can yet scarcely hope that for many generations astronomy will look upon their like again.

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* That Sir John Herschel never withdrew the opinion that that fact is demonstrated by the evidence, I happen to know quite certainly; because, commenting on a remark in my 'Other Worlds,' which seemed to imply that he had changed his mind, he noted in a letter to myself that he still retained the opinion expressed in the passage referred to.

† That this did, at any rate, sometimes happen, cannot be denied even by Sir John Herschel's warmest admirers, since in the preface to his 'Outlines of Astronomy,' we find him noting that theories which he had spoken of as 'certain curious views of M. Jean Reynaud' had been 'reasoned out' by himself 'to identical conclusions' many years before, a fact which had 'completely escaped his recollection when perusing the works of M. Reynaud.'

The St. Paul's Magazine for June 1871.

THE STUDY OF ASTRONOMY.

THE death of the great astronomer to whom more than to any other we owe the interest with which astronomy is studied in our time, invites us to some reflections on the value of such study, and on the special purposes which it is best fitted to subserve. I wish particularly to note that I am not here about to examine the utilitarian aspect of the science. No one is likely to dispute the assertion that in our highly utilitarian age the practical application of astronomy subserves highly important purposes. The whole system of commerce, for example, depends on the accuracy with which the astronomers of Greenwich and other national observatories note the apparent motions of the stars. The survey of land districts cannot be efficiently carried out without astronomical observations and a careful consideration of astronomical principles. And besides a number of other instances in which astronomy is directly applied to practically useful purposes, it is only necessary to consider how many and what important interests depend on the commercial relations between different countries, and on the careful survey of the earth's surface, to see that astronomy holds almost as high a position among the useful sciences as among those which relate chiefly to the extension of our knowledge. But, as I have said, it is not of the utilitarian aspect of astronomy that I wish to speak. I purpose to consider the study of astronomy as a means of mental training,—whether as affording subjects of profitable contemplation; or as offering problems the inquiry into which cannot fail to discipline the mind; or, lastly, as suggesting the actual application of methods of observation by which at once the patience and ingenuity of the observer may be exercised, his knowledge extended, and his mind supplied with fresh subjects for study.

For whatever those may think who have not familiarised themselves with the teachings of astronomy, there can be no question that the highest place is given by astronomers themselves to those rather who have advanced our knowledge of astronomical facts—whether by careful observation or by judicious theorising—than to those who have applied astronomy most successfully to practical purposes. If we take the names which are most highly honoured by astronomers, and consider why they are honoured, we shall see that this is so. I suppose that practical astronomy, as it is now known to us, would have had no existence but for the researches of Copernicus, Kepler, and Newton. It is true that the same amount of labour devoted to the simple observation of the celestial movements might very well have resulted in making astronomers quite as confident both in prediction and retrospection as they actually are. But it is altogether unlikely that the same amount of labour would actually have been directed to astronomical inquiries but for the confidence engendered by the work of Copernicus, Kepler, and Newton. So that in one sense we may say that these great men have done more to advance practical astronomy than any others, and that the high honour in which their names are held by astronomers would be justified by this circumstance alone. Yet, if we rightly consider the labours of Copernicus, Kepler, and Newton, we shall find that they were by no means primarily directed to practical astronomy. Their effect in advancing the study of practical astronomy may be regarded as, in a sense, accidental; or rather this result affords an illustration of the fact that, in scientific research, we need not keep continually before our minds the question '*Cui bono?*' since a good which the student of science himself may not perceive will commonly result from even the least promising researches. We know that Copernicus only sought to explain observed appearances by a simpler theory than that which was in vogue in his day. To Kepler, perhaps, the idea may have suggested itself that the laws he sought for so earnestly, in order to explain the movements of Mars as traced by the best observational methods yet applied, might result in giving to astronomers a new power of predicting the motions of Mars and the other planets. But certainly the object which Kepler set himself was to replace the disorder of the Ptolemaic system and the but partial symmetry of the system of Copernicus, by a harmonious series of relations. When he had succeeded, his boast was, *not* that he had shown astronomers how henceforth they might confidently predict the motions of the celestial bodies, but that he had 'found the golden vases of the Egyptians.' Nor is it possible to read Newton's own account of those researches by which the law of gravitation was established without feeling that, to himself at least, the practical application of the law in after-times was of secondary import. It was the law itself, regarded as a discovery respecting the manner in which the bodies distributed throughout space influence and are influenced by each other, which he valued.

If we turn our thoughts to the astronomy of the past century, we recognise the same fact. It would be difficult to find in the whole of that noble series of papers which Sir William Herschel contributed to the pages of the 'Philosophical Transactions' a single paragraph directed to the application of astronomical discoveries to practical purposes. And whether we consider those discoveries which are commonly but erroneously supposed to constitute Herschel's chief title to honour, or those which astronomers regard as his most valuable contributions to science, we find in either case that we have to deal with discoveries which have, primarily, no practical value whatever. For example, the discovery of Uranus, which so many suppose to have been Herschel's noblest work, was undoubtedly full of interest, but it certainly was not a practically useful discovery. And, again, to turn to that which was in reality the noblest work achieved by Herschel—his researches into depths lying far beyond the range of the unaided vision—in what sense can the counting of myriads of stars or the discovery of thousands of nebulæ be regarded as advancing in the slightest degree the material interests of mankind? Even if it hereafter happened that the discovery of Uranus or the processes of star-guaging should indirectly lead to some practical results of value, it would still remain certain that Sir William Herschel had had no such results in his thoughts when he prosecuted his researches.

In our own time Sir John Herschel has been justly held by all to be the leading astronomer of his day; yet it would be difficult to find in a single astronomical research of his the least practical value; while certainly in that long series of observations on which astronomers base their high opinion of him, there was no practical value whatever. Sir John Herschel had already devoted eight years of his life to the re-examination of his father's work, with the chief end of acquiring a mastery over his telescope, when at the Cape of Good Hope he began a series of observations which formed the exact counterpart of his father's observations in the northern skies. Star-guaging, the noting of double stars, the search for nebulæ—all these lines of research must needs advance the science of astronomy, but not one of them has any practical utility.

Nor, even if we take the well-merited fame of depart-mental astronomers—if we may so distinguish the workers in special branches from men who, like the Herschels, have made all astronomy their subject—can we recognise the title to such fame in practically useful work. When Adams and Leverrier by subtle processes of research showed astronomers where to turn their telescopes to detect the planet whose influence had disturbed the motions of Uranus, they were not in any way advancing the material interests of the human race. It may happen, indeed, that some of the mathematical processes devised or developed by these great men may one day be applied in some practical manner; but no one will, on this account, assign such practical results as the real title of Adams or Leverrier to astronomical fame. Even the practically useful work of an Airy or a Hind is not that which is regarded among their fellow-astronomers as affording their chief claim to honour.

In considering astronomy as a subject of study, the first point to which we must direct our attention is the mode in which astronomical discoveries should be presented. I wish particularly to invite attention to the reasons of Sir John Herschel's great success in attracting the minds of men to a subject which, before his time, had been regarded as too recondite for general study. I wish to consider why it is that those facts which before his day seemed bewildering rather than impressive, became in his hands the means of attracting hundreds to the study of his favourite science. Herein I have to deal with the workings of my own mind; for, recalling my impressions of astronomical facts as presented by those works in which I first studied the science, and comparing those impressions with my feelings in regard to the science after I had read Sir John Herschel's 'Outlines of Astronomy,' I find between my earlier and later views all the difference that exists between listlessness and earnestness.

The secret of Herschel's success I take to be the fact that he is never content with merely stating such and such circumstances about the celestial bodies, but will not leave his subject until he has impressed on the mind of his reader his own feeling of the reality of those circumstances. It would be easy to multiply examples of this characteristic peculiarity of his method of teaching; one, however, will suffice, and I take it almost at random:—

He has described the actual relations of certain double stars; and so far as the facts respecting these objects are concerned, the reader has already had presented to him all that is necessary. Then, in that singularly effortless manner with which he always passes from description to imagery, he proceeds thus: 'It is not with the revolutions of bodies of a planetary or cometary nature round a solar centre that we are now concerned—it is with that of sun round sun; each, perhaps, at least in some binary systems where the individuals are very remote and their period of revolution very long, accompanied with its train of planets and *their* satellites, closely shrouded from our view by the splendour of their respective suns, and crowded into a space bearing hardly a greater proportion to the enormous interval which separates *them*, than the distances of the satellites of our planets from their primaries bear to their distances from the sun itself. A less distinctly characterised subordination would be incompatible with the stability of their systems and with the planetary nature of their orbits. Unless closely nestled under the protecting wing of their immediate superior, the sweep of their other sun in its perihelion passage round their own might carry them off, or whirl them into orbits utterly incompatible with the conditions necessary for the existence of their inhabitants. It must be confessed that we have here a strangely wide and novel field for speculative excursions, and one which it is not easy to avoid luxuriating in.'

I have spoken of the absence of effort which characterises the introduction of such passages as these; and I take it that this absence of effort is absolutely essential to their effect. It is only when such passages are perfectly natural —natural not merely in appearance, but in reality—that they arouse the full sympathy of the reader. And their influence in this last respect might be taken as no unsafe test of their *being* purely natural effusions. But in the case of Sir John Herschel we have the means of proving, in an independent manner, that his most poetical descriptions were written, not to display his powers, but because they came unbidden to his pen. We have the records of his observations as made in the stillness of night, with no thought but to represent what he had actually seen; and among these records we come again and again upon passages which no one familiar with Sir John Herschel's descriptive style could for a moment fail to recognise as his. Here, for example, are a few of his notes respecting the lesser Magellanic Cloud: they are taken from the Guagebooks: 'The access to the Nubecula Minor is *on all sides through a desert.*' 'The lesser Nubecula is now approaching, but I discern no indications in the field leading me to expect any remarkable object: on the contrary, the *stippled* appearance noted shortly before is gone, and the ground is black. The ground of the sky is completely black throughout the whole breadth of the sweep. The body of the cloud is fairly resolved into excessively minute stars, which, however, are certainly seen. It is a fine, rich, large cluster of very small stars, which fill more than many fields, and is broken into many knots, groups, and straggling branches, but the whole is clearly resolved.' Then, after passing the limits of the cloud, 'here is *a region of utter barrenness—a miserably poor and barren region—most dreary* since the small Nubecula.' Take also this sketch of a nebula, and the accompanying suggestion as to the constitution of certain regions of space, as affording evidence of the style of Herschel's note-books: 'A beautiful nebula; it has very much resemblance to the Nubecula Major itself as seen with the naked eye, but is far brighter and more impressive in its general aspect, as if the Nubecula were at least doubled in intensity. And who can say whether in this object, magnified and analysed by telescopes infinitely superior to what we now possess, there may not exist all the complexity of detail that the Nubecula itself presents to our examination?'

I believe that it is only by presenting astronomical facts in this striking and graphic manner that they can be made acceptable to the generality of readers. This is true, indeed, in all sciences; but it is specially true of astronomy, since there is no science where the facts are on the one hand so wonderful in reality, or on the other so capable of becoming unimpressive, and even wearisome, if not earnestly dealt with.

Yet let me in this place note that there is a fault of a different nature than want of earnestness, which equally requires to be avoided in scientific treatises. I refer to the undue familiarity of tone by which sometimes even our ablest expositors attempt to descend to the presumed level of their readers' comprehension. Even Sir John Herschel, it must be admitted, has sometimes condescended to express himself in too familiar terms when dealing with subjects which require grandeur of treatment. Not, indeed (so far as I remember), in his 'Outlines of Astronomy,' at least in the main text of that noble work; but in some of his Essays one is certainly somewhat startled at times by a familiarity which does not seem suited to the nature of the subject-matter. For example, I think that, without being hypercritical, the astronomer may fairly object to some points in the following passage, in which Sir John Herschel is speaking of the sun's attractive energy: 'Even in his capacity as ruler, the sun is not *quite* fixed. If he pulls the planets, they pull him and each other; but such family struggles affect him little. *They amuse them*' (the italics are not mine), 'and set them dancing rather oddly, *but don't disturb him*.' Nor again can one accept altogether with satisfaction that passage in which, after speaking of a comet as of a restive horse, Herschel remarks, of the first three observations made on a comet, that 'the third nails it.'

The fact is that Sir John Herschel shows his real power as a scientific writer only when he deals grandly with grand subjects. Through this power he was unrivalled as a populariser of science. But in the less dignified *rôle* of a familiariser he was not successful. His gambolling was that of Behemoth. Nor, indeed, would his failure in this respect require notice, were it not that many have been led to follow his example in precisely that matter in which it was least desirable that he should be imitated. For instance, his fashion of calling the solar prominences 'things' by way of expressing their doubtful nature, has been followed as carefully as if it were an ornament rather than a blemish of his style. And one might readily cull from the writings of those who have imitated Herschel's familiarity, passages which he assuredly would have shuddered at.

It is not merely necessary that astronomical facts should be so presented to the student that he may become possessed with a feeling of their reality, but the student cannot be rightly said to 'have astronomy' at all (to use Shakespeare's apt expression) until he is capable of picturing to himself, however inadequately, the truths of the science. A man may have at his fingers' ends the distances, volumes, densities, and so on of all the planets, the rates at which they move, the physical features they present, and a hundred other facts equally important; but, unless he has in his mind's eye a picture of the solar system, with all its wonderful variety, and all its yet more amazing vitality, he has not yet passed even the threshold of the science. He must be able to conceive the mighty mass of the sun, ruling from the centre of the scheme the whole of that family to the several members of which he distributes their due proportion of light and heat. Close around the sun the student must see the family of minor planets; small Mercury lit up with inconceivable splendour by the sun, round which he speeds with unmatched velocity; Venus and Earth, the twin planets of the solar system, alike in all features, save only that Venus has no satellite; and lastly, ruddy Mars, the miniature of our own earth. Then beyond the path round which Mars urges his course, the student must picture to himself the interlacing paths of hundreds of asteroids, tiny orbs compared with even the least of the minor family of planets, yet each pursuing its independent course around the sun, many doubtless approaching almost within hail (if one may so speak) of their fellow orbs, and many free to depart far more widely than any of the primary planets from the general level near which the planetary motions are performed. Then, lastly, he should picture to himself that wonderful outer family of planets, the least of which exceeds many times in bulk the combined volume of all the minor planets and asteroids. The vast globe of Jupiter circled about by his symmetrical family of satellites, the complex system of Saturn, with his gorgeous ring-system and a family of satellites the outermost of which has an orbit range of more than four and a half millions of miles; Uranus and Neptune, brother orbs, almost lost in the immensity of their distance—all these planets, and all the wonders which the telescope has taught us respecting them, should be clearly pictured. In particular, the enormous distances separating the paths of these bodies from each other, and from the sun, should be clearly apprehended, and that strangely incorrect picture which defaces so many of our books on astronomy, wherein the paths of the planets are seen separated by nearly equal distances from each other, should be as far as possible forgotten. When the student has apprehended the fact that the whole family of the minor planets could not span the distance between the orbits of Jupiter and Saturn, while the distance between the orbits of Saturn and Uranus, or of Uranus and Neptune, almost equals the full span of the orbit of Jupiter, he has already made an important step from mere book knowledge, almost useless (in itself), towards that clear recognition of actual relations which should be the true end of scientific study.

But beyond the solar system the thoughts of the student of astronomy should range until he begins to apprehend to some extent the vastness of those abysms by which our solar system is separated on all sides from the realm of the fixed stars, that is, of the orbs which are the centres of other systems like itself. And I know of no consideration which tends more clearly to bring this idea before the mind of the student than the thought that our sun, with his attendant family of planets, is speeding through those abysms with a velocity altogether past our powers of conception, while yet no signs of his motion, and our motion with him, can be recognised, even after the lapse of centuries, save by taxing to the utmost the powers of our noblest telescopes. The clear recognition of this fact, and of its real significance, enables the thoughtful student to become conscious of the vastness of the depths separating us from the nearest fixed star, even though he can never form an adequate conception of their tremendous proportions. That within the abysm which forms his present domain our sun traverses in each second four or five terrestrial miles, while yet he seems always to hold a fixed place in that domain,—this is the great fact which serves most strikingly to impress upon us the vastness of the interstellar spaces.

There is another, however, which deserves mention. We commonly find those comets which sweep round the sun in parabolic or hyperbolic orbits, spoken of as visitants from the domain of other stars. And so in truth they are. But how seldom do we find in our treatises on astronomy any reference to the enormous intervals of time which must have elapsed since these startling visitants were travelling close round some other star, making their periastral swoop before setting forth on that enormous journey which had to be traversed before they could become visible to our astronomers! Taking into account the directions in which certain comets have reached us, and assigning to the stars seen in such directions the least distances compatible with known facts, it yet remains absolutely certain that twenty millions of years at least must have elapsed since those comets were last in periastral passage. While if, as some suppose, each comet (even those which now circle in closed orbits round our own) has flitted from star to star during a long interstellar existence, the mind shrinks utterly before the contemplation of the vastness of the time-intervals which have elapsed since those journeyings first commenced: yet these time-intervals afford but an imperfect means of estimating the scale on which the sidereal system is built.

I will not dwell here on those further conceptions—equally necessary, I take it, to complete the picture which the true student of astronomy should have present in his mind—which relate to the constitution of the sidereal spaces, to the motions and changes taking place within them, and to the relation which the various forms of matter existing within those spaces bear to each other, or to the forms with which we are familiar. It is to be remarked, as regards many of these conceptions, that their nature will depend on the views entertained by the student respecting the accuracy of the various theories which Kepler, Wright, Kant, Lambert, Mitchell, the Herschels, Struve, and others, have formed respecting the way in which the various objects revealed by the telescope are distributed throughout surrounding space. But even though doubt must needs at present rest on many points, yet what is actually known is sufficient to form a picture full of interest as respects all its visible details, and not the less impressive, perhaps, that a large portion of its extent is still hidden in darkness and mystery.

It is little necessary to point out that the course of study by which astronomical relations may thus become clearly pictured must needs form a valuable mental training. Whether we regard the careful analysis of the evidence on which astronomical facts rest, the study of the various facts as they are brought, one after another, to the student's knowledge, the due co-ordination of each with its fellows, or, finally and chiefly, that *intention* of the mind on the complete series of facts by which alone their real significance can be apprehended, we see in astronomy the apt means for disciplining the mind, and fitting it for the noblest work of which it may be capable. But, besides the study of astronomical facts, we must consider here the actual study of the heavens, either with the unaided eye or with the telescope. I speak of the study of the heavens with the unaided eye, though many in this age of cheap telescopes may be inclined to smile at the thought that such study can have any value either to the student or to the science of astronomy. As a matter of fact, however, I am of those who believe that much may still be learned even from the study of the stellar heavens without optical instruments of any sort. I would point, in corroboration of this view, to the work done by Argelander in this seemingly so limited field; to our still incomplete knowledge of the meteoric facts which naked-eye survey is capable of revealing; and, lastly, to the fact that, from the study and charting of those stars alone which are visible to the unaided eye, I have myself been led to results tending to render untenable the whole system of sidereal astronomy as presented in our text-books.* I need hardly say that I reject altogether the notion that a telescope of even moderate power must needs be useless because in our day there are so many powerful telescopes, mounted in well-fitted observatories, and in the hands of men who are certainly not ill qualified to carry out original investigations.

Now I think that nothing can exceed in value the practical study of astronomy by the direct survey of the heavens. Setting aside the fact that it is in the student's power to add to our store of knowledge, it is of the utmost importance that he should become directly cognisant of astronomical facts, whether those facts be the seeming motions of the celestial bodies, the telescopic aspect of the sun, moon, planets, stars, and nebulæ, or the statistical relations, changes, motions, and so on, of the stars of various orders. A student of astronomy whose knowledge is partly founded on actual observation holds all his knowledge with far securer grasp than one who has devoted his attention, however earnestly, to the acquisition of book-knowledge alone.

Yet I find it impossible to pass this point of my subject without a word of protest against the use to which the telescopes now erected in every part of England are, with few exceptions, being devoted. One can understand that a person who has been led by the study of astronomical works to possess himself of a telescope of greater or less power, would in the first place turn it as opportunity permitted towards the various objects of which his books have informed him. One can understand that he would tax the powers of his instrument in attempting to recognise the spots on Venus or Mars, the more delicate details of lunar scenery or of the sun's surface, the belts of Jupiter, the features of the Saturnian rings, the duplicity of the closer double stars, and the characteristics of those exceedingly difficult objects of study, the *nebulæ.* But it certainly does seem a misfortune either that the work should stop here or that work of this sort should be continued year after year without aim or purpose. Yet in one or other of these ways, not merely the hundreds of cheap telescopes at this moment in the hands of amateur observers, but numbers of the finest telescopes which our Cookes, and Brownings, and Dallmeyers have turned out from their manufactories, are simply lost to the cause of astronomy. A fine instrument is purchased, and erected in a well-fitted and costly observatory; and during the first weeks after its erection the purchaser turns it on some of the objects he has read about. Then presently his enthusiasm is exhausted, and the telescope is no more used, save perhaps to amuse visitors. Or, else, the telescopist's enthusiasm waxes fiercer; he passes night after night in his observatory, making his life a burden by unceasing efforts to just see with his telescope what one a little larger would show him easily; he sets his clocks and watches and all his neighbours' clocks and watches by transit observations; he notes down (to the second or third decimal place of seconds) the epochs when the moon occults stars or when Jupiter's satellites are eclipsed or occulted; and he seemingly remains all the while unconscious of the fact that twenty times his misplaced energy devoted for twenty lives to such work as I have described would produce results simply worth *nothing.*

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* Of course, the weight of this evidence will depend on the eventual acceptance or rejection of the views which I have founded on the above-mentioned researches. But whether my views be accepted or rejected (and I must frankly state that I have not the least anxiety as to their fate), the facts I have brought forward *must* be explained; and however explained, they must bear to a greater or less extent on our theories respecting sidereal astronomy. The aggregation of stars in certain regions, and their segregation from others, for instance, may be regarded otherwise than I regard these facts; but the facts are there, and they have resulted from the survey of that which so many mistakenly suppose to be an exhausted region of astronomy—the relations, namely, presented by objects visible to the unaided eye.

This rule I suggest to every possessor of a telescope as one which should be written in letters of gold in his observatory, or, rather, as one which should be kept continually in his thoughts while working there: *Every observation not intended as a mere relaxation from real work should be intended to ascertain some as yet unknown fact.* Grant that the fact sought after may turn out when found to be an unimportant one, or even that after much labour no new fact may be revealed at all. In any long series of researches it must needs happen again and again that labour is wasted. But there is all the difference in the world between labour wasted unavoidably, and the deliberate employment of time and labour in purposeless observations. Bernard Palissy wasted years of labour, and all but ruined himself, in seeking to master the secrets of pottery; yet his successive failures were justified by his final success—nay, they would have been justified by his purpose even though he had failed; but no reasoning can justify the successful labours of the man who constructed a carriage complete in all its parts, which the wing of a fly could completely cover. The true astronomer finds it difficult to forgive the telescopists who successfully imitate the work done at Greenwich in systematic observatory work of the most utterly valueless nature, while he can admire the unsuccessful labours of Sir William Herschel directed to the inquiry whether the planet Uranus has rings.

It will be obvious that careful attention to the rule I have stated above will not merely lead to the devisal of new applications of telescopic power, but is likely to suggest to the ingenious observer new ways of supplementing the powers of his telescope. It is only necessary to consider the various contrivances suggested by that prince of modern observers, the late Mr. Dawes, to see how, without very heavily taxing his inventive or constructive powers, the observer may enter on researches which his telescope as it came from the hands of the maker would not have enabled him to carry out successfully. Nor can one study the labours of any of our more successful observers without seeing how very readily new researches may be effected by contrivances of extreme simplicity.

I would next invite attention to the absolute necessity of independence of mind in the study of the noblest of all the sciences. I would not indeed advocate a readiness to dispute the dicta of the great men who have devoted themselves to the advancement of astronomy; nor again is it fitting that the student should attempt to make independent inquiries into matters belonging to such branches of the science as he has not yet familiarised himself with. It is neither dispute nor cavil that I advocate, but the careful examination and analysis of all statements submitted to the student's consideration, and the attempt to render the subject as far as possible his own by such a survey of the evidence as will suffice to give him independent reasons for believing in the correctness of the conclusions of his teachers. It will not unfrequently happen that while thus engaged he will detect, or imagine that he has detected, errors of greater or less importance. He should be prepared to find that in most cases these seeming errors have no real existence, but arise from misapprehensions on his own part—a circumstance which will of itself serve to convince him of the extreme importance of the kind of investigation by which such misapprehensions have been brought to light. But in other instances he will find that there has been a real error in his text-book—a fact which will equally convince him of the importance of the careful analysis of all statements lying within his range of investigation.* I would quote here the words of Professor Huxley, both as to the value of scientific doubt, and as to the nature of that sort of doubt which the student should alone permit himself: 'There is a path that leads to truth so surely, that any one who will follow it must needs reach the goal, whether his capacity be great or small. And there is one guiding rule by which a man may always find this path, and keep himself from straying when he has found it. This golden rule is, "Give unqualified assent to no propositions but those the truth of which is so clear and distinct that they cannot be doubted." The enunciation of this first commandment of science consecrates doubt. It removes doubt from the seat of penance among the grievous sins to which it had long been condemned, and enthrones it in that high place among the primary duties which is assigned to it by the scientific conscience of these latter days.' But 'you must remember that the sort of doubt which has thus been consecrated is that which Goethe has called "the active scepticism, whose whole aim is to conquer itself;" and not that other sort which is born of flippancy and ignorance, and whose aim is only to perpetuate itself as an excuse for idleness and indifference.'

I have not hitherto referred specially to the grandeur of the facts with which the student of astronomy becomes acquainted. Certainly in this respect Astronomy stands before all other sciences. Geology alone approaches her in respect of the vastness of the time-intervals which either science presents to our contemplation. But as respects extension in space, the domain of geology is utterly insignificant by comparison with even the threshold of that vast domain into which astronomy invites us. The geologist's field of research is indeed, as the most distinguished living geologist has remarked, 'insignificant when compared to the entire globe of the earth;' and astronomy teaches us to regard that globe, and even the system to which it belongs, as occupying the merest speck of space by comparison with the visible portion of the star-system; while the sphere enclosing all the stars visible to the naked eye is small by comparison with the spaces revealed by the telescope, and infinitely small by comparison with those spaces whose existence is suggested by telescopic research. Nor is even the vastness of the domain of astronomy the noblest feature of the science. The wonderful variety recognised within that domain is perhaps but faintly pictured in the solar system with all its various forms of matter—sun, primary planets, and moons; major planets, minor planets, and asteroids; planet-girdling rings, meteoric systems, and comets; with perchance other forms of matter hitherto unrecognised. And beyond the wideness of the domain of astronomy and the amazing variety recognised within that domain, there remains the yet more impressive lesson taught by the infinite vitality which pervades every portion of space. I apprehend that if such powers of vision, and also (for they would be even more needed) such powers of conception, were given to the astronomer that the extent of that domain which the telescope has revealed to man could be adequately recognised, while he further became cognisant of the way in which the various portions of that domain are occupied, that, deeply as he would be impressed by the amazing scene, the sense of wonder he would experience would sink almost into nothingness by comparison with that which would overwhelm him could he recognise with equal clearness the movements taking place amongst the orbs presented to his contemplation—could he see moons and moon-systems circling around primary planets, these urging their way with inconceivable velocity around their central suns, while amid the star-depths the suns were seen swiftly travelling on their several courses, star-streams and star-clusters aggregating or segregating according to the various influences of the attractions to which they were subject, and the vast spaces occupied by the gaseous nebulæ stirred to their inmost depths by the action of mighty forces whose real nature is as yet unknown to us. The mind cannot but be strengthened and invigorated, it cannot but be purified and elevated, by the contemplation of a scene so full of magnificence, imperfect though the means be by which the wonders of the scene are made known to us. The information given by the telescope is indeed but piecemeal, and as yet no adequate attempts have been made to bring the whole array of known facts as far as possible into one grand picture; but, seen as it is only by parts, and (even so) only as through a veil and darkly, the scene presented to the astronomer is the grandest and the most awe-inspiring which man can study.

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* The necessity of such inquiry is increased by the circumstance that too often the statements made in one work on astronomy are repeated without modification or examination in others, thence to be requoted in other works with, perhaps, fresh errors due to misprints, misapprehension, &c. For instance, I have noticed that in a popular text-book of astronomy, from misapprehension alone, two out of three methods of determining the longitude have been wrongly described, and *in three several instances* the actual reverse of the truth has been asserted in the explanation of so simple a matter as the equation of time. May it not be questioned how far it is just that those who have still so much to learn should undertake to write text-books of science?

Fraser's Magazine for September 1871.

THE PLANET MARS.

Of the planets within the orbit of Uranus, Mars appears, at first sight, to be the least inviting object of study to the observer armed with moderate telescopic power. Jupiter, from the noble aspect of his disc, and the ever-varying configurations of his attendant orbs, is among the most charming of telescopic objects. With a telescope of somewhat higher power than that available for the study of the larger planet, Saturn bears away from him the palm for splendour of appearance, and for the wonderful yet symmetrical beauty of his attendant system. Venus and Mercury, in a lesser degree, although both are 'difficult' objects, yet attract the young observer, by the lowness of the powers with which their varying phases are made conspicuously visible. Mars, on the other hand, presents no features which a telescope of very low power can reveal; and even with a telescope of considerable power, some patience, combined with skill and practice in observation, are required to enable the observer to interpret satisfactorily the phenomena presented to him. Yet it must not be forgotten that, of all the planets, Mars is that which is in reality the most favourably situated for telescopic research; or, rather, it would not be saying too much to assert that Mars is the only object in the heavens whose examination is capable of supplying an answer to some of the questions which most largely interest the thoughtful mind. With the telescopes yet constructed, indeed, it were too much to hope that very exact information as to the physical condition of Mars should be gleaned, under whatever circumstances the planet may be observed; nor would the simple increase of magnifying power, which the past history of the telescope leads us to hope for and expect, conduce greatly to the attainment of the above-named object. But it does not seem too much to hope that some day (haply not so far distant) the lesson taught us by Professor Smyth's Teneriffe experiment will be appreciated as it deserves. Then a telescope surpassing in power any yet constructed shall be placed where alone the power of such an instrument can be efficiently exerted—where Newton long since told men that such an instrument should be placed—far above those denser atmospheric strata whose disturbances never cease, and are magnified and aggravated by every increase of telescopic power. When this is done, we may look in Mars for that which has long been sought for fruitlessly upon the lunar surface—the signs of life, of change, of progress, of decay. In one point, indeed, Mars has already supplied such evidence; since, as we shall presently see, he exhibits, in regular succession, appearances corresponding to changes well known to be taking place regularly upon our earth.

There is another circumstance which tends to heighten the interest with which the astronomer regards this small planet. Its motions, watched for many long years by Tycho Brahe, and studied for twenty years by the ingenious Kepler, were the means of overthrowing for ever the elaborate system of errors and hypotheses known as Ptolemaic astronomy. They afforded also to Newton the first hint on which he founded the law of universal gravitation. The figure of Mars's orbit, and the relation which that orbit bears to the orbit of our earth, rendered the planet the most fitting, one may almost say the only fitting, member of the solar system for the purposes Kepler had in view.

DRAWINGS OF MARS.

N Hanhart li

As it is necessary for the right understanding of the appearances presented by Mars at successive returns to opposition that the nature of his orbit should be rightly understood, I shall solicit the reader's patience while I run as briefly as possible through the points of chief importance. This is the more necessary because no popular work on astronomy (that I at least have ever met with) presents with any approach to accuracy this very important feature of the solar system. Even that admirable and interesting work, Guillemin's 'Heavens,' deals very inadequately, though at some length, with this question.

In Plate II., $E_1E_2E_3E_4$ represents the orbit of the earth, and $M_1M_2M_3M_4$ that of Mars. M is the perihelion of Mars's orbit, which, it will be observed, is noticeably eccentric (C_1, the centre, being 13,000,000 miles from the sun), M′ the aphelion; E is the perihelion of the earth's less eccentric orbit (whose centre is at C_2), E′ the aphelion. The arrows indicate the direction in which both planets revolve around the sun. The plane of Mars' orbit is inclined at an angle of 1° 51′ 5″ to that of the earth, the points marked ☊ and ☋ being those at which the orbit of Mars intersects the plane of the earth's orbit; at M_x and M_s Mars attains his greatest distance from the plane of the earth's orbit, the short arrow indicating, as nearly as possible, on the scale of our figure, the distance at which Mars is above and below the plane of the ecliptic at these two points respectively. Of the absolute dimensions of the two orbits, it will be sufficient to say that the greatest and least distances of the earth from the sun are respectively 93,190,000 and 90,110,000 miles, the greatest and least distances of Mars 152,670,000 and 126,620,000 miles.

Mars takes 686·979 days in completing one circuit around the sun; thus it is easily calculated that the mean interval between successive oppositions is 779·836 days. Owing, however, to the great eccentricity of Mars's orbit, and the consequent considerable variation in the rate of his motion around the sun, the successive synodical revolutions of the two planets vary in length, being greater or less according as opposition occurs near perihelion or near aphelion respectively. The positions of the oppositions from 1867 to 1881, marked in Plate II., will be sufficient to indicate this. The line of opposition travels round in the order of the signs. After travelling *twice* round the zodiac, the line falls very nearly in the position it had at starting, such double revolution occupying thirty-three years, in the course of which Mars has been fifteen times in opposition.

It will be obvious, from a moment's inspection of Plate II., that the appearance presented by Mars, when in opposition near M,* must be very different to that presented when he is in opposition near M′: the distance of Mars in the former case being less than his distance in the latter case in the proportion of about 19 to 37; or, in miles, the former distance is 34,140,000, the latter 61,860,000 miles. Hence arise variations in the magnitude of the disc presented by the planet; and since Mars in perihelion is more brilliantly illuminated than when in aphelion, his apparent brightness is yet further increased. By the first cause his brightness is increased as the squares of the numbers 37 and 19, and by the second as the squares of the numbers 41 and 34; or, on the whole, his brightness, when in opposition in perihelion, is about five times as great as his brightness at opposition in aphelion. So bright does he appear, when the first conditions are nearly approximated to, that his appearance has caused alarm to the uneducated. Theoretically, indeed, he ought to appear brighter at such times than Jupiter himself at his brightest, since the disc of Mars, smaller than that of Jupiter in the ratio of 24 to 49, is more brilliantly illuminated in the greater ratio of 472 to 126, so that Mars should appear brighter than Jupiter in the ratio of about 5 to 3. Jupiter, however, sends us more light, probably because his atmosphere bears large belts and masses of clouds capable of reflecting light very perfectly, and also preventing the loss of light which would accrue in the double passage through the planet's atmosphere. The studies of our leading astronomers and physicists leave little doubt that the light by which we see Mars has suffered diminution in this way to a very considerable extent.

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* Owing to the eccentricity of the earth's orbit, and the circumstance that the perihelia of the two orbits have different positions, M is not absolutely the point of Mars's orbit which lies nearest to the earth's orbit. The point of nearest approach *precedes* M by a small arc, which (were it worth while) it would be easy to calculate.

Oppositions of Mars near perihelion occur at intervals of fifteen and seventeen years successively. Sometimes it happens, as in 1860 and 1862, that two successive oppositions occur at nearly equal distances from perihelion; it follows that the next opposition near perihelion (in 1877) will fall midway between these positions, or very much nearer perihelion than either of the two others; in other words, Mars will be very favourably situated for observation in 1877. Much of the superiority of perihelion-oppositions is, however, lost in our northern latitudes, since these oppositions occur in August; and the sun being high by day, it follows, of course, that the ecliptic (near which Mars is always situate) is low by night. On the other hand, it is clear from the figure that, if Mars is in opposition in midwinter, when of course he has a considerable altitude at night, he is too near aphelion to be favourably seen. On the whole, it follows that the most favourable season in which Mars can be in opposition, is towards autumn (when he is near ☊ of Plate II.). At this season, while not very far from perihelion, he attains an altitude of from 55° to 60° on the meridian. Such an opposition took place in 1862, when very admirable views of Mars were obtained by Messrs. Dawes, Lockyer, and Phillips, and by others of our best observers. The opposition of 1864 was also a very favourable one.

But another circumstance remains to be considered. The planet, rotating on an axis considerably inclined to the plane of the orbit (and also to the ecliptic), presents at different seasons different aspects, not only with reference to the sun but also to the observer on the earth. At one time his north pole is bowed down towards the sun, at another his south pole; and the same relations, only in a somewhat more complex order, are maintained with respect to the earth. If, then, the astronomer would rightly study the peculiarities of our neighbour Mars, he must examine the planet at oppositions occurring in every part of its orbit.

As respects the inclination of Mars's axis to the plane of his orbit, and the other elements on which his seasons and the appearance he presents to us depend, we have the determinations of Sir W. Herschel. He estimated that the North-Martial spring occurs when the planet is in longitude 79° 28′ (the longitude indicated in the figure is 78°); the obliquity of the Martial ecliptic he set at 28° 42′; and the inclination of Mars's equator to the earth's orbit at 30° 18″. I fear it will be considered somewhat rash to impugn results obtained by Sir W. Herschel. Standing, as he does, in the very foremost rank among observers, and *facile princeps* as an *interpreter* of observations, astronomers justly look on his opinions almost as laws. Yet I think, if we consider the nature of the observation, and the character of the instruments used by Herschel, we must admit the fact that he attributed to his results an exactness they were not capable of possessing. The pictures of Mars given by Herschel are sufficient to show that the instrument he used was far inferior in defining power to those with which De la Rue, Dawes, Lockyer, and Phillips have examined the planet. Now, let us see on what indications furnished by Mars (thus viewed) Herschel founded the determinations above recorded. Referring to the paper in the 'Philosophical Transactions,' * we find that the indications he trusted to were the motions of spots across Mars, and the appearance or disappearance of certain bright spots near the Martial poles. In fact, from the nature of the case, it is obvious that no other sort of evidence was available. The necessary observations were repeated at intervals, as the weather permitted, and carefully reduced (on just mathematical principles) in accordance with the motions of Mars and the earth in their respective orbits. Now, if we consider the minuteness of the disc presented by Mars, the variable appearance of the spots and points upon his surface, and the extreme difficulty of assigning, with any approach to exactness, the period or place at which a spot or point becomes visible on the edge of a rotating sphere, even when such sphere is distinctly (and permanently) marked, we shall see that, even with the best modern instruments, it would be impossible to determine the inclination of Mars's axis within two or three degrees, or the place of his vernal equinox within seven or eight degrees. Those who are best able to appreciate Herschel's work as an astronomer will be precisely those who will most clearly recognise the difficulty of the problem he attacked. It is to be wished that some of our modern observers would re-examine the subject. That very little attention has been bestowed upon it by writers on astronomy will be evident from this, that the numbers given by Herschel are repeated, not only without comment, but even without those changes which the variations in the orbit of the planet render necessary. Given the position of Mars's axis with respect to his orbit, and the position of his orbit with respect to the earth's, then the position of his axis with respect to the earth's orbit follows at once. If either of the

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* *Phil. Trans.* 1784, p. 241.

data vary, the result will vary. Now, the second datum has varied largely since Herschel's time; but no corresponding variation in the angle 30° 18′ (named above) has been introduced into our works on astronomy.

The diameter of Mars is differently estimated by different astronomers. In Mädler's 'Elements,' 4,070 miles is assigned as the planet's equatorial diameter. Most observers assign a larger diameter: Hind, in his 'Astronomy,' giving the planet a diameter of 4,500 miles. These estimates are, of course, founded on the old estimate of the sun's distance. It seems probable that 4,150 miles on that estimate, or 4,000 miles, if the modern reduced estimate of the sun's distance is accepted, is not very far from the true diameter of the planet. In other words, the linear dimensions of Mars are about one-half those of the earth, or twice those of the moon. More roughly, his surface is about one-fourth that of the earth, or four times that of the moon; and, yet more roughly, his volume about one-eighth that of the earth, or eight times that of the moon.

Herschel determined the compression of Mars at $\frac{1}{16}$. Modern observers greatly reduce this quantity. Professor Kaiser, of Leyden, makes the compression $\frac{1}{114}$; Main, of the Radcliffe Observatory, deduced $\frac{1}{39}$ in 1862, but in some earlier measurements made the polar greater than the equatorial diameter. Mr. Dawes, applying two modes of measurement, found, from the first, no compression; from the second, he found the polar greater than the equatorial diameter. Is it going too far to say that the oblateness of Mars' figure is not yet determined satisfactorily? Probably it is too small for measurement.

Herschel made Mars's rotation-period 24 h. 39 m. 35 s.; Mädler gives 24 h. 37 m. 23·7 s.; and Professor Kaiser considers 24 h. 37 m. 22·6 s. the true value. My own estimate of the rotation-period is 24 h. 37 m. 22·735 s. (see Appendix A).

It is not to be assumed that Mars presents at all seasons identical features. It is found, in fact, that, besides periodic changes in the dimensions of those two white caps near the polar regions which have so long been recognised as

The snowy poles of moonless Mars,

the details of other portions of his surface vary from time to time. Spots and patches clearly made out on one occasion appear blurred and indistinct on another—though the same telescope may be used, and our own atmosphere (as tested by the performance of the telescope on double stars) may be in a state as well suited for definition. The colour of the planet is also variable; the redness (compared to a faint tinge of Indian red by some observers, and to a coppery tint by others), and the greenish-grey tint of the darker parts of the disc, being much more marked on some occasions than on others. Another phenomenon—the paleness of the disc round the edges—is also variable.

The variations in the appearance of Mars are clearly explicable on the natural hypothesis of an atmospheric envelope, such as that surrounding our own earth, bearing clouds and mists over the surface of the planet. Judging by the analogy of our own earth, we may consider that the planet's cloud-covering would vary in density not only from place to place upon the surface, but, considered as a whole, from season to season, and from year to year. It gives a high idea of the difficulty of the problem attacked by astronomers, in examining Mars, to note that, for favourable research, we must have a fine night upon our earth, and a clear day on Mars, combined with favourable circumstances of distance, altitude, and presentation; that we cannot watch the planet through any single Martial year, but must be content to piece in the observations of different seasons of different years; and, finally, that Mars, when in opposition at the solstice of one of his hemispheres, is nearly twice as far from us as when in opposition at the corresponding solstice of the other hemisphere.

Notwithstanding these difficulties, many excellent drawings of Mars have been taken by astronomers, the planet being shown in almost every possible presentation. Among those who have distinguished themselves in such work must be mentioned Sir W. Herschel, Messrs. Beer and Mädler, Kunowski, Lockyer, Phillips, and De la Rue. The drawings obtained by the late Mr. Dawes surpass, however, all others in interest. Being desirous of charting the planet, I ventured to apply to Mr. Dawes for tracings of a few drawings taken when the planet was presented in various ways to us. With the kindness for which he was so remarkable, and which endeared him so much to all who became acquainted with him, he immediately sent me ten or twelve drawings, and afterwards searched through his note-books for others. In all, if I remember rightly, he sent me twenty-one drawings, taken in 1852 (a most valuable series in this year), in 1856, in 1860, and in 1862.

The task of charting Mars from these drawings was not so easy a one as might at first sight have been supposed. Mr. Dawes had taken them at various hours, and there was no ready means of determining the position of the planet's axis in each case. A tentative process had to be gone through —for I was anxious that the charting of Mars should be independent of all previous efforts in that direction.

Having calculated the presentation of Mars for the date of each drawing, I drew on tracing-paper the meridians and parallels properly presented (on the scale—in each case—of the corresponding drawing by Mr. Dawes). Then beginning with the most promising view, I placed the tracing-paper over the picture of the planet, giving that position to the polar axis which corresponded most closely with the assigned position of the polar snow-caps. Then on a projection of the meridians and parallels of a globe on the equidistant projection, I drew in the lands and seas of Mars as they appeared under the meridian-lines on the tracing-paper. I next repeated the process for other drawings in which the same features were presented.

At first there was little accordance between the results thus pencilled on my chart-projection. This was caused by erroneous selections of the axial line of Mars, which—it must be remembered—does not correspond with the position of the polar snow-caps. But gradually I began to get over this difficulty, and the views began to show a much closer agreement. Still there were slight discrepancies, and these when reduced as much as possible by shifting the assumed position of the axis, I was obliged to ascribe to such slight errors as could not fail to appear in drawings so full of detail and taken under such circumstances of difficulty as were Mr. Dawes' pictures. Therefore, having drawn in all the outlines deducible from pictures nearly approaching each other in phase, I considered a *mean outline* taken through the others to be as nearly as possible correct.

It must be understood that the amount of Mars's surface covered by one such series of processes would be very much less than a full hemisphere, since—firstly, the part of Mars near the limb was not drawn in so distinctly in Dawes' pictures as the rest, and secondly, a small mis-drawing in an orthographic presentation of a planet becomes much more important as we leave the centre of the disc, so that I did not consider myself justified in using those delineations which were not near the centre. It must also be remembered that, as the drawings were not taken at periods separated by regular intervals of Martial time, it was very necessary to apply to each a correction calculated according to the true value of Mars's rotation-period. Thus it will be understood that before the whole of the surface of Mars had been charted a considerable amount of labour had been given to the subject. Those who have never tried work of this sort would hardly be able to conceive how perplexing it often becomes.* But one circumstance was very pleasing. I found that the more carefully I worked at the chart, the more thoroughly the true value of Mr. Dawes' drawings came out. I had had little conception, when I began the work, either of the acuteness of his vision or of the accuracy of his powers of delineation. The tracings he sent me were partially covered with faintly marked streaks which I had at first supposed to be merely random touches thrown in to indicate the general appearance of that part of Mars to which they belonged. But I soon found that every one of these streaks was to be taken as the indication of a Martial marking which Mr. Dawes had actually seen. The strange variations of figure which a spot on a globe undergoes when the globe is looked at in various directions, had prevented me at first from recognising the identity of several large markings. Mr. Dawes himself was not aware, in some cases, that a spot which was presented with one figure in one drawing was in reality the same as one which appeared with a totally different figure in another drawing. But when due account was taken of the effects of foreshortening, the almost perfect correspondence between the different views, indicated at once the accuracy of Mr. Dawes' drawing, and the permanence of the spots which mark the globe of Mars.

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* Soon after the above statement appeared, an attempt was made by a writer in the *Athenæum* to hand over the results of my labours to my friend Mr. Browning. The latter had, at my suggestion, made a globe from my equidistant chart of Mars; and he had exhibited at the Royal Society some beautiful stereograms of this globe. The globe itself had been exhibited at the meeting of the Astronomical Society in May 1868, with sufficient reference to the source whence its features had been copied. But the writer referred to, in describing the photographic pictures of the globe, spoke of them as derived by Mr. Browning from Mr. Dawes' drawings. It appeared to me desirable to correct what I at the time regarded as a mere oversight. For, my part of the work had been by no means light, and without it Mr. Dawes' drawings, beautiful though they were, had given very little information as to the *areography* of Mars. My reclamation was not well received. I was gravely assured that I had not done what I supposed; but that what I had really done was to determine the rotation-period of Mars. On my pointing out that this work was distinct from the other, which was *also* mine (my chart being published five months before its features were reproduced, without the slighest modification, in Mr. Browning's globe), the anonymous writer asserted that his original statement was quite correct; but he suddenly found that the matter was not worth disputing about, though he had been warm in his laudations when handing over my work to another. I should add that he had also assigned to Mr. Browning the credit due to Prof. Phillips, of having been the first to construct a globe of Mars. This I corrected, as also did an anonymous correspondent.

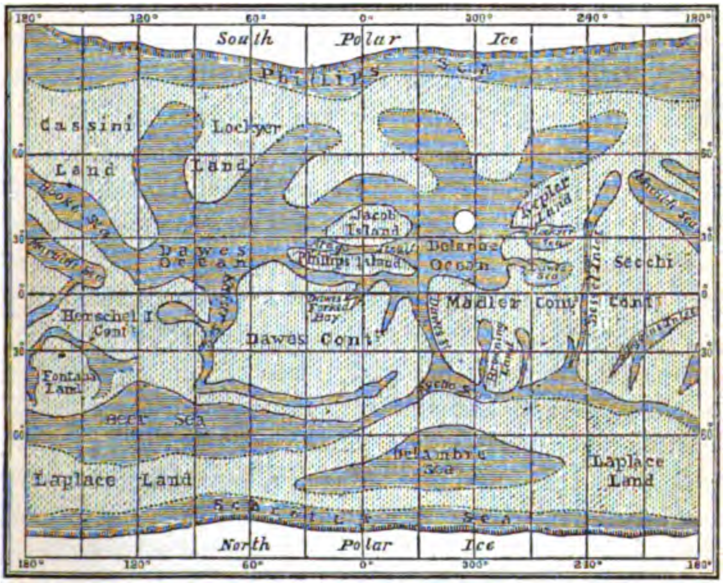

Fig. 1. Chart of Mars on Mercator's Projection.

The result was the construction of a chart of Mars containing a number of features which had not before appeared in works of the sort. In my 'Half-hours with the Telescope,' (Plate VI.), a small copy of the equidistant chart originally drawn by me is presented. Fig. 1 represents the same features on Mercator's projection.*

A feature of the planet Mars which has attracted some attention has been incidentally noticed above. I refer to the whiteness of the disc near the limb. This phenomenon is worthy of a careful examination; and I believe that the true explanation has not yet been put forward.

In the first place it is to be remarked that this phenomenon is *real* and not merely apparent. The edge of Jupiter's disc seems to be brighter than the central part, but is in reality darker. I believe ninety-nine observers out of a hundred would be deceived regarding this feature of Jupiter, if they trusted to the unaided eye. Why it should be so is not perhaps very easy to say. Perhaps the contrast between the dark background of the sky and the illuminated limb of the planet tends to give to the latter a brightness which does not belong to it. Be this as it may, a series of observations which Mr. Browning has lately made of Jupiter, with the express object of determining this question, has resulted in placing the greater darkness of the planet's limb, as compared with the central part of the disc, beyond a doubt. He used darkening glasses perfectly graduated from end to end, and by this means was enabled to obtain the most accurate estimate of the relative brilliancy of various parts of the disc.

But the greater darkness of Jupiter's disc near the limb is what was theoretically to have been expected. An opaque globular body directly illuminated by a distant luminous orb should appear brightest in the centre of its disc; because the real illumination diminishes as the angle at which the light-rays meet the surface diminishes, and the apparent brilliancy at any point of an object is always equal to the real illumination at the point.

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* In my *Other Worlds* will be found a coloured chart of Mars on the stereographic projection.

In the case of Mars, then, the apparent illumination of different parts of the disc varies in a manner which is directly the reverse of what was theoretically to be expected. Therefore, it behoves us to determine with so much the greater accuracy whether the eye may not be deceived in this as in the former case. I believe the experiment applied by Mr. Browning to Jupiter's disc has never been applied to that of Mars. But, fortunately, a series of photometrical experiments by Dr. Zöllner, although not directed to the question we are considering, but to the determination of the total amount of light received from Mars at different epochs, yet affords a satisfactory reply to our doubts. For it will be easily understood that when a globe is not illuminated strictly according to the usual law—but, from some reason unknown, presents an anomalous variation of brilliancy—the total amount of light received from it at different times will not correspond with the estimate deduced according to the usual law. For example: the moon's light at full does not bear to the moon's light at the quarter the proportion which would exist if the moon were a perfectly smooth globe, and therefore illuminated strictly according to the law mentioned above (in dealing with Jupiter). And by assuming—what is practically the case—that the illumination of the hemisphere of Mars turned towards the sun varies according to some law depending merely on the distance from the central point of that hemisphere, it follows that, by noting the amount of light received from Mars at different times—and especially by comparing the amount received from him in quadrature, with that received when he is in opposition—it becomes possible to deduce the law according to which different parts of his disc are illuminated. For although when Mars is in quadrature his gibbosity is not very remarkable, yet the true centre of the illuminated hemisphere is removed a considerable distance from the centre of the disc, and the total illumination is therefore affected in a remarkable manner by the planet's gibbosity.

Accordingly, Zöllner was able to estimate the anomalous illumination of various parts of Mars's disc. He had already done this in the case of the moon, and had come to the conclusion that the anomalies in the lunar illumination (mean) are due to the existence of irregularities over the moon's surface, and he estimated the mean angle of inclination of the slopes of the lunar mountains to be somewhat over fifty degrees. Assuming that the same explanation held in the case of the anomalies of Martial illumination, he found that the surface of Mars must be covered with mountains having a slope of about seventy-six degrees.

But this view is surely untenable. We can accept Zöllner's explanation in the case of the moon; in fact we may almost say that it is obviously the true one. We can conceive no other cause available to produce the effect considered, and further we see that all over the moon there are mountains having very steep sides. But in the case of Mars we cannot admit such an explanation, because a large part of the surface of the planet appears to be covered with water, and because also a slope of seventy degrees and upwards is outrageously steep. Mars ought to be covered all over with hills shaped like sugar-loaves to account for his anomalous illumination in the way suggested by Zöllner.

To me a far more natural way of explaining the difficulty seems to be the following. We have every reason for believing that clouds form over the surface of Mars as over that of the earth. Secchi, Dawes, Lockyer, and Browning agree in describing effects which can scarcely be due to any other cause. And besides we shall presently see that there is good reason for feeling absolutely certain that the vapour of water exists in large quantities in the atmosphere of Mars. Now, it would not be a very bold speculation to argue from the observed anomalies in the illumination of Mars, that clouds prevail much more towards (Martial) morning and evening than in the middle of the day. If this were so, it would, of course, follow that the parts of Mars which as seen from the sun lie near the edge of the limb, would be much more brilliant than the rest. For they are the parts where it is morning or evening with the Martialists; therefore, according to the assumption, they are cloud-covered; but clouds reflect much more light than the solid or liquid surface of Mars; therefore these parts of the disc would seem proportionately more brilliant.

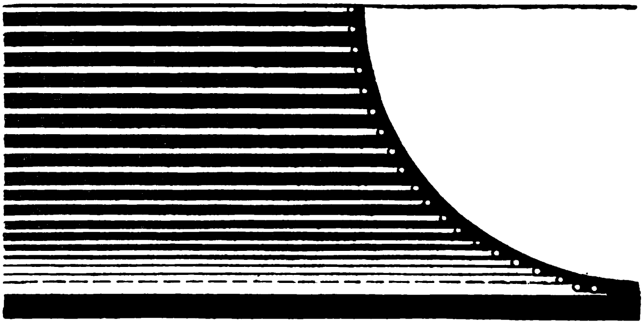

FIG. 2. Explaining the brightness of the edge of the disc of Mars.

But we are not even required to make such an assumption as this. For if clouds were pretty uniformly distributed over the whole surface of Mars there would still result a greater brilliancy of the limb. Consider Fig. 2 for example. Here a fourth part of the circumference of Mars is supposed to be illuminated by the sun on the left, and clouds are represented which are arranged with perfect uniformity all round this quadrant. When the light falls between the clouds, it is supposed to be returned after a considerable absorption, corresponding to the shaded spaces. When it falls on a cloud, it is supposed to be returned after much less absorption—that is, to remain much more brilliant after reflection—corresponding to the unshaded spaces. And it is at once seen that near the limb all the light is (in this imaginary case) derived from reflection at the clouds, whereas, near the centre of the disc, the larger proportion is derived from reflection at the real surface of the planet.

There is nothing doubtful in the above explanation, except the assumed existence of small clouds—invisible separately to the naked eye. But this assumption seems at once more natural, and to explain the difficulty better than the sugar-loaf mountains of Zöllner.

It may be, however, that when the sun is near the horizon of Mars, heavy mists hang in the air, as happens commonly enough, with us, both in the morning and in the evening. This would account equally well for the observed peculiarity.

I should be glad to hear that anyone armed with a telescope of adequate power had done something to test the climatic relations of Mars, and also the diurnal changes in the state of the Martial atmosphere. By noticing at what part of the disc the features appeared most distinct (allowance being made for real differences in the distinctness of the markings), something might readily be done in this way. The spectroscope also might be rendered very efficiently available in this inquiry. It has been already noticed by observers that the winter hemisphere is perceptibly less distinct on the whole than the summer hemisphere. But then, as there are places on earth where the winter climate is drier than elsewhere, so it may be that parts of the winter hemisphere of Mars may be more distinct than others. In considering diurnal changes account must be taken of the gibbosity of Mars at the time of observation, because, as we have said, the centre of the disc of Mars may be far removed from the centreof the illuminated hemisphere.

Perhaps the most remarkable discovery yet made respecting the physical condition of Mars, is that contained in a communication addressed to the Royal Astronomical Society, by Mr. Huggins, early in the year 1867. From this paper I extract the following particulars.

On several occasions during the opposition of 1867, Mr. Huggins was able to make observations of the spectrum of the planet's light, or, to use his own accurate phraseology, 'of the solar light reflected from the planet.' During these observations he saw groups of lines in the blue and indigo parts of the spectrum. But the faintness of this part of the spectrum did not permit him to determine whether these lines are the same as those which occur in the same part of the solar spectrum, or whether any of them are new lines due to absorption undergone by the light at reflection from the planet.

He also detected (as in former observations) several strong lines in the red part of the spectrum, and it is to these that the chief interest of his paper attaches. He saw Fraunhofer's *é* very distinctly, and another line about one-fourth of the way from C towards B. As the latter line has no counterpart in the solar spectrum, it was clearly due to an absorptive effect produced by the planet's atmosphere. On February 14, Mr. Huggins was able to detect faint lines on both sides of Fraunhofer's D. These lines occupied positions in the spectrum apparently coincident with groups of lines which make their appearance in the solar spectrum, when the sun is low down—so that its light has to traverse the denser strata of the atmosphere. It remained however to show that these lines were produced by the atmosphere of Mars, and not by that of our own earth. This Mr. Huggins effected in the following manner:—The moon was, at the hour of observation, somewhat lower down than Mars, so that if the lines were due to the absorptive effects of our atmosphere, they should have been more distinctly marked in the spectrum of the lunar light than in that of the light from Mars. But when the spectroscope was directed to the moon these lines were not visible, thus conclusively proving that the lines were caused by the absorptive action of the Martial atmosphere. Mr. Huggins noticed in confirmation of this that the lines seemed more distinct in the light from the margin of the disc, but he was not quite certain on this point.

This observation proves the presence of aqueous vapour in the atmosphere of Mars, since the lines in question have been shown to be caused, in the case of our atmosphere, by the vapour of water.

From the spectroscopic analysis of the darker portions of the disc of Mars, Mr. Huggins was led to the conclusion that these parts are neutral or nearly so in colour.

He considers also that the ruddy colour of Mars is not due to the effects of the planet's atmosphere. Indeed, this seems almost obvious when we consider that the polar spots look perfectly white, or at least show not the slightest tinge of red, although, being situated upon the edge of the disc, they should exhibit the effects of the atmosphere's absorptive powers more strongly than the central parts of the disc, where the light has passed through a much smaller range of atmosphere. Clearly we may look upon the red colour of parts of Mars as due to the nature of the planet's soil.

Abridged from Papers in the *Popular Science Review* for January 1867 and January 1869.

SATURN'S RINGS.

There is no object in the heavens which is so well calculated to excite our admiration as the planet Saturn, when observed with a good telescope. The nebulæ exhibit to us systems which are in reality incomparably more magnificent. The double stars, rightly understood—and especially those binary systems whose periods extend over many hundreds of years—afford stronger evidence of the grand scale on which the universe is created. But the evidence which Saturn affords is more readily appreciated. The mind must be dull, indeed, which does not recognise at once, in the splendid architecture of the Saturnian system, the fashioning power of the great laws which the Creator has set His universe. The beauty of the system, the perfect regularity of the gigantic rings, the delicate varieties of colour which the practised observer can detect both in the planet and its attendant ring-system, and the magnificent scale on which all these features of interest are exhibited, attract and impress the attention; while the singular problems suggested by the stability of the rings, or still more by the slow processes of change to which they appear to be subjected, invite the exercise of the fullest powers of the observer and of the mathematician.

I propose now to consider some of the discoveries which have been recently made respecting Saturn's ring-system, and to suggest some processes of observation which, if well carried out, might afford valuable information on the subject of the rings.

I shall assume a knowledge on the reader's part of all those features of the Saturnian system which are usually described in treatises on astronomy. Nor shall I enter at any length into the circumstances which have led astronomers to recognise, in the system of rings, the presence of a multitude of discrete particles or minute satellites, revolving for the most part in one plane around the globe of the planet. I must make one or two preliminary remarks on this interesting hypothesis, however, lest some portions of what follows should not seem intelligible to those who may not happen to be familiar with the views now received.

It had been shown, by Laplace, that the stability of the motion of such rings as were supposed to surround Saturn could only be maintained by a considerable over-weighting of one portion of each ring, and an equally remarkable eccentricity of position. Later astronomers, admitting this view as the basis of their inquiry, came to the conclusion that the disturbing action of the satellites might cause a balancing motion in the ring-system, sufficient at least to secure stability,—somewhat as the slight motions by which a rod is balanced in an upright position, although these motions are severally opposed to the rod's stability, yet, by their united effect give to the rod a comparative fixity of position which the most perfect quiescence of the support could not secure. These views maintained their ground until the discovery of the dark ring, and of the strange fact that the planet's body could be seen through this formation without apparent distortion. The discovery of this ring led to a renewed examination of the problem; and finally Professor Maxwell of Cambridge proved, by a most convincing process of mathematical demonstration, that no solid ring could by any possibility continue to exist as an attendant upon a planet. Either the ring would crumble into fragments under the influence of the forces to which it would be subjected, and these fragments would continue to revolve as a broken ring round the planet; or the ring would be more completely destroyed, and would be brought to the planet's surface. Hence we are forced to conclude that the rings, though continuous in appearance, consist of flights of minute bodies, each travelling on its own orbit around the planet.

But although to the mathematician capable of following Professor Maxwell through all the processes of a complicated proof, the demonstration of the satellite theory of the rings may seem complete, there can be no doubt that the more convincing evidence of observation is wanted to bring the fact home to the mind of the general student. Now we cannot hope that the most powerful telescopes which man can construct will suffice to reveal the separate bodies which form the ring. When the ring's edge is turned towards us it appears as an almost evanescent line of light, and doubtless if its figure had not length as well as breadth, we could not detect any trace of its existence. Yet there is every reason to believe that the apparent breadth of that fine line of light is many times larger than the apparent diameter of any single satellite belonging to the rings. In this way, then, observation is not likely to help us.

But there is a mode in which evidence might be gathered respecting the conformation of the rings, by any observer who had patience to conduct the requisite series of observations.

If we consider the case of a series of flat rings (whose thickness may be neglected) formed as the rings of Saturn were once supposed to be, we shall see that the apparent brilliancy of the rings ought to vary with the amount of opening. We do not refer to the total amount of light received from the ring, but to the apparent brilliancy of any point upon the system. When a plain surface is illuminated, the science of optics tells us that the illumination is proportional to the cosine of the angle of incidence. In fact, we know from experience that the higher the sun is above our horizon the greater is the amount of light received on the earth's surface around us. Precisely so would it be with the rings if they had plane surfaces. And further, it is a law of optics that the apparent brilliancy of any point of a luminous object is equal to the real brilliancy at that point, whatever may be the distance of the object, or the angle at which the line of light meets the surface (neglecting always—what does not here concern us—the influence of any absorptive medium which may be interposed between us and the object).

Now, this being so, it is very evident that if the rings were flat the total amount of light received from them (the ball being supposed removed) would be increased, through *two* causes, as the rings opened. First, the increased apparent size of the luminous surface would have an obvious effect. Owing to this cause the illumination would vary as the sine of the angle at which the line of light from the earth is inclined to the plane of the rings. Secondly, the apparent brilliancy of each point of the ring-system would be increased as the sine of the angle at which the sun's rays are inclined to the plane of the rings. Thus the total amount of light would increase as the product of these two sines, or assuming what is commonly the case, that the earth and sun are almost equally raised above the surface of the rings, the total amount of light received from the rings would vary as the square of either sine.

But if the rings consist of a multitude of discrete satellites, there must result a different state of things. Take a single satellite, and we see at once that so long as the whole of this satellite can be seen we get the same amount of light from it, whatever the elevation of the sun above the mean plane of the rings. And though the problem seems to get somewhat complicated when we consider the case of a multitude of satellites, yet it will be found, on examination, that there is no longer the same variation to be looked for as was shown to exist in the former case, owing to the sun's change of elevation. In fact, we have a case somewhat resembling that of the moon; the illumination of whose disc has been shown by Zöllner not to diminish towards the edges according to the varying inclination of the solar rays to the moon's surface, but rather to increase; while calculation has shown the probable reason to consist in the fact that the moon is not a smooth globe, but covered with hills and mountains, whose sides are inclined at greater or less elevations to the mean level of the lunar surface.

This being so, two means of observation seem available. First, a definite part of the ring's width might be compared with the equatorial bright belt of the planet; the brilliancy of that belt being we may assume constant. This method would probably involve difficulties; but from the success with which Mr. Browning guaged the relative brilliancy of different parts of the disc of Jupiter last spring, I have no reason to doubt that, with suitably prepared and graduated darkening glasses, the comparison might be satisfactorily carried out: then the change of brilliancy of the particular part of the ring examined, as the system gradually closed, would afford evidence of the nature of that portion of the ring, according to the principles enunciated above. Secondly, a process might be applied to Saturn, corresponding to that which Dr. Zöllner recently applied to the planet Mars. By determining the total amount of light received from Saturn at successive oppositions, and deducting therefrom that portion which calculation (founded on the light received from the planet when the ring disappears) shows to be due to the globe, it would be possible to determine according to what law the ring varies in brilliancy as its amount of opening changes, and thus to determine generally what may be the nature of the ring's surface.

The result of the application of spectroscopic analysis to the rings has been at once interesting and perplexing. The spectrum of the planet's light exhibits certain absorption-lines indicative of the presence of vapour. Now Mr. Huggins has discovered that the same lines are present in the spectrum of the ring's light also; and that, of the two, the latter spectrum exhibits these dark lines somewhat the more distinctly. This result is remarkable. It indicates that the amount of vapour through which the light from the globe has passed before reaching us is less than the amount passed through by the light from the rings. We are accustomed to recognise the probability that the globe of Saturn is surrounded by an atmosphere proportional in extent to the enormous volume of the planet. On the other hand, the small bodies forming the rings, if they had atmospheres at all, would have vaporous envelopes so limited in extent, one would suppose—the volume of each of these satellites being so minute—that the most powerful spectroscope should fail to reveal any trace of its existence. Supposing them to resemble our own satellite, but on a much smaller scale, their atmospheres would be a million-fold too small to produce any distinctive dark lines in the spectrum of their light. For though the moon is so much the nearest of all the celestial bodies, her spectrum has no dark lines other than those belonging to it as formed by reflected solar light. When we remember that Saturn, when at his least distance from the earth, is upwards of 820 millions of miles from us, or more than 3,000 times farther from us than the moon is, the visibility of distinctive dark lines in the spectrum of the ring will appear one of the most interesting and remarkable results of spectroscopic research. It would be perplexing in the extreme if we supposed the rings to be continuous bodies; but accepting, as we are bound to do, the theory that they consist of flights of minute satellites, the result becomes one of the most surprising that can well be imagined.

The explanation I would venture to offer of this strange phenomenon will, I fear, appear to many unduly speculative, if even it do not seem opposed to well-known physical laws. In an appendix to my treatise on Saturn, I have maintained the view that the moon has so thoroughly parted with its original internal heat that even the gases once subsisting on its surface have been transformed into the solid form. I was aware when I so wrote, that at the time of full moon the hemisphere we see (or a part of that hemisphere) is subjected to a heat exceeding that of boiling water. An enormous amount of heat poured in this way upon the surface of a planet would be rendered latent in transmuting but a small portion of the solidified gases into the aërial form, and produce no effects observable to us on earth; just as the full heat of a tropical summer's day poured for hours on the peaks of the Himalayas, produces no change which the inhabitant of the valleys can perceive, on the snowy masses lying there. If this view were just, we should learn to look upon all the satellites throughout the solar system as in a somewhat similar state to that of our own moon; and at first sight the members of the Saturnian rings would appear, on account of their extreme minuteness, to be of all others those in which the cold would be most intense. But then a circumstance comes to be considered which would have an effect the other way.

It is a part of the theory of the motions of satellite-rings, that there would be continual collisions among the members. I have shown in full, in Chapter V. of my treatise on Saturn, how these collisions would arise and how they would operate upon the figure of the ring-system. There would be a gradual increase of width, chiefly through the approach of the inner edge of the rings towards the planet; and there would also be a tendency to the formation of new rings within those already formed. But the true significance of these changes is this, that the whole system must be continually undergoing a loss of *vis viva.* Every collision involves such a loss, and the increase in the width of the system is in a sense a measure of the amount of loss. But this increase of width, though indicating, does not compensate for, the loss of *vis viva.* There is only one way in which the loss can be compensated, and that way is indicated in a passing manner, in a note at p. 126 of my treatise on Saturn. There must be a continual generation of heat corresponding exactly to the loss of *vis viva.* Now this heat must tend to render the condition of all the satellites of the system very different from that of one of the ordinary attendants upon a planet. For all must partake in the distribution of this heat; because it is absolutely impossible that any single satellite can have an orbit which, even for a few hours, can keep it free from collision with one or more of its fellows. Thus every satellite is kept warm, so to speak, by a process of continual friction, and no such process of refrigeration as I conceive to have taken place upon the moon, can come into operation upon the satellites forming Saturn's rings. Nay, it may well be that the heat of these bodies is very much greater than the mean heat of our earth's surface. For processes of collision fully equal to the generation of such heat might be in operation without appreciably affecting the apparent width of the ring-system. And certainly the present appearance of the dark ring is such as to encourage the view that sufficiently rapid changes are in progress.

It would follow from these views, that the spectrum of the ring's light would exhibit variations corresponding to the various parts of the ring's breadth. Of course, there are already well-marked gradations of light in the spectrum, because the light is different in different parts of the ring's breadth. But the dark lines I have already spoken of as distinctive of the ring's spectrum, ought to be more distinctly seen in certain parts of the ring on another account. For there can be little doubt that the central parts of each ring are those at which collisions take place most frequently between the satellites; and, therefore, if the cause I have been considering is really in operation, the dark lines ought to be seen best in those parts of the spectrum's width which correspond to the central portions of the rings. The observation might be worth making, though it would be one of great difficulty and delicacy.

Some recent researches by Professor Kirkwood, of Illinois, have supplied an interesting and sound proof of the real structure of the rings. They are particularly interesting to myself, as affording an unexpected proof of a view I had put forward some time since which had seemed to some to be more imaginative than well-founded. In the preface to my treatise on Saturn, I had said that possibly we may yet detect in the Saturnian rings the indications of those processes by which the solar system had reached its present state. Now Professor Kirkwood's researches tend directly to establish such a relation.

He had shown that when the asteroids are arranged in the order of their mean distances, certain well-marked gaps are observable, and that these gaps correspond to those mean distances which would give periods commensurable with the period of Jupiter. We know that when a planet has a period very nearly commensurable (according to some simple relation) with the period of a neighbouring planet, the two bodies disturb each other much more effectively than they would if there were no such relation. If one of the planets be much larger than the other, far the larger part of the disturbance falls upon the motions of the smaller planet. Saturn, for example, had long since been noticed as having his motions affected by a very remarkable inequality; and the search for a cause resulted in the discovery that the peculiarity is due to the relation existing between the motions of Saturn and Jupiter, by which two revolutions of the former planet are accomplished in about the same time as five of the latter. The disturbance falls principally on Saturn, as being so much the smaller of the two bodies. And as the asteroids are exceedingly minute when compared with Jupiter, it is evident that those members of the system which had periods commensurable with his would be very largely disturbed, and so come to have *another* period. Thus we can understand the fact that there should be no asteroids at those particular mean distances from the sun which correspond to the particular periods in question.

But it is clear that if there were any possibility of doubting the fact that the asteroids form a zone of disconnected bodies, the circumstance established by Professor Kirkwood would prove that fact. If, then, we can trace in the Saturnian ring-system any signs of the action of similar processes, we shall have an independent and perfect proof that the rings are not continuous, but composed of discrete satellites. Now this is precisely what Professor Kirkwood has been able to do. He has shown that a small satellite revolving in the space between the outer and inner rings—that is, travelling around the black division—would have a period commensurable not merely with that of the neighbouring Saturnian satellite, Enceladus, but with those of all the four inner satellites. It remains absolutely certain, therefore, that the ring is composed of bodies moving freely in definite orbits. And, further, those who agree with me in accepting the nebular hypothesis (or a modification of it) as truly representing the mode in which the solar system reached its present condition, will see, in the law established by Professor Kirkwood, the action of one of the processes which must have been most effective in the formation of our system.

This paper would be incomplete if I did not refer to the information which Mr. Browning, F.R.A.S., has recently obtained respecting the variety of colours observable in the Saturnian system. I had never been able to recognise any well-marked signs of colour on Saturn with a four-inch achromatic refractor.* But not only has Mr. Browning himself been able to detect a variety of tints with his large reflector, but I have seen a letter from an observer (using a similar but smaller instrument) who refers to the same tints. These tints are thus compared by Mr. Browning with the well-known colours of the paint-box:—

'The rings yellow-ochre, shaded with the same and sepia. The globe yellow-ochre and brown madder, orange and purple, shaded with sepia. The crape-ring, purple madder and sepia. The great division in the rings, sepia. The pole and the narrow belts, situated near to it on the globe, pale cobalt blue. These tints are the nearest I could find to represent those seen on the planet, bnt there is a muddiness about all terrestrial colours, when compared with the colour of the objects seen in the skies. These colours could not be seen in their brilliancy and purity, *unless we could dip our pencil in a rainbow, and transfer the prismatic tints to our paper.*'

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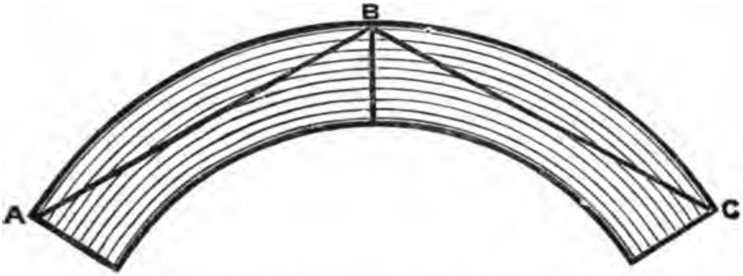
* It must be remembered that small apertures are more favourable, as a rule, for the exhibition of colour than large ones. In the case of Saturn, perhaps, the rule should rather be, 'large apertures and high powers.'

With reference to these interesting and graphic remarks, it must be pointed out that we might reasonably be disposed to refer phenomena so new and so remarkable to some peculiarity either of the telescope or of the observer's vision, were it not that the observed blueness of the polar regions at once negatives such a supposition. I cannot but think the evidence thus afforded of the adaptability of reflectors to delicate chromatic studies singularly striking and convincing.

The shadow of the planet on the ring (see frontispiece) is an interesting subject of observation. Singular and as yet little understood peculiarities of form have been exhibited by this shadow. The contrast between the blackness of the shadow and the colour of the so-called black division between the rings, is also well worth noticing. If any doubt could remain respecting the constitution of the rings, no argument could be more effectually used in favour of the satellite theory than that drawn from the fact that the division between the rings is not vacant, but occupied by some entity or other which supplies a faint but readily detected light. I cannot conceive what reasonable theory could be urged in explanation of this peculiarity, save that some minute bodies are travelling within the dark division.

Popular Science Review for July 1869.



SATURN'S SQUARE-SHOULDERED ASPECT.

INSTANCES have been given of optical illusions affecting our estimate of the relative *size* of figures placed in particular positions with respect to each other. In the figure accompanying this article a somewhat similar illusion affecting *shape* is illustrated; the lines A B and B C, which appear to have a decided curvature, being in reality straight lines.

In figures constructed on a larger scale, and with concentric circles closer together (in proportion), the deception is still greater; and it is remarkable that the illusion is increased by drawing equidistant lines radiating from the centre of the concentric circles. I notice, also, that a want of symmetry in the drawing seems to destroy the illusion.

The deception struck me as remarkably perfect in the case in which I first observed it. I had drawn the meridians and parallels for a polar star-map on the equidistant projection, to the scale of an 18-inch globe—the parallels to every degree, and the meridians, from the 20th parallel of N. P. D. to the bounding parallel (N. P. D. 37° 23′) of the map, also to every degree. Thus the map formed a circle 11¾ inches in radius, with 37 concentric circles crossed by 360 radiating lines, drawn with as much uniformity as possible. Now, before marking in stars, I wished, as a matter of curiosity, to determine the exact figure, on the *equidistant* projection, of the spherical pentagon which in my *gnomonic* maps appears as a true pentagon. I accordingly drew in, in pencil, first the inscribed pentagon, and then (through points determined by their known R. A.'s and N. P. D.'s) the five curved sides of the figure I required. Thus the sides of the true pentagon formed chords of the five sides of a curvilinear pentagon *outside* the true one. But now I could scarcely persuade myself that I had not by mistake drawn the convexities of the curves the wrong way; in other words, that the curvilinear pentagon I had drawn was not a true pentagon, and its sides the chords of a curvilinear pentagon *inside* the true one. I had, in fact, to take a tracing of the curvilinear pentagon before I could form a satisfactory conception of its real shape.

This illusion seems to have a direct bearing on the question of the *square-shouldered* figure sometimes assumed by Saturn. We see that a series of concentric, similar, and symmetrically disposed curved lines give to a straight line crossing them an appearance of curvature in a direction opposite to that of the curved lines. Hence a line drawn with a certain slight curvature in the same direction as the curvature of the concentric lines would appear straight, and a line with a greater curvature would appear to have its curvature diminished. Further, if such a line were continued beyond the concentric lines, the alteration of curvature would disappear at a short distance from the concentric lines. Hence ♅'s observation of the flattening of Saturn's equator, and his determination of a maximum diameter and curvature at latitude 43° (not far from the apparent intersection of the ring's outer boundary with the outline of the disc), seems to be satisfactorily explained. Yet it cannot be denied that there are grave objections to the optical explanation of the phenomenon. One would expect that the illusion would be perceptible in pictures of Saturn; that it would be always observable; or, if it be supposed to depend on the extent to which the rings are open, that it would always be noticed when the rings are open to a particular extent; that it would disappear when the rings are closed; that it would not affect micrometrical measurement —or, if it affected measurement in one case, that it would do so in all cases. A perusal of the evidence contained in the second edition of my 'Other Worlds' (pp. 161–165) will suffice to show that not one of these conditions is fulfilled.

I venture with great diffidence to offer some considerations which seem to point to a different solution of the difficulty.

If we assumed, either that the atmospheres bear any proportion to the masses of planets, or that any provision is made by increased depth of atmosphere for diminution of solar heat, we might fairly suppose that the height of Saturn's atmosphere is nine or ten times as great as that of our own atmosphere. Now the earth's atmosphere has been supposed to extend to a height of from 100 to 150 miles (Nichol's 'Cyclopædia of the Physical Sciences,' Art. 'Atmosphere'); but taking the more moderate estimate of fifty miles, the height of Saturn's atmosphere might be assumed to be from 450 to 500 miles.* Our positive evidence is not, perhaps, very strong. The circumstance that the constitution of an atmosphere so far removed admits of determination by spectrum-analysis seems to indicate that the height of the atmosphere must be considerable. Mr. Grover's observation of a penumbra surrounding the planet's shadow on the ring ('Astronomical Register' for August 1865, p. 212), 'always noticed' even with the moderate telescopic power applied by this observer, also points to the existence of a strongly refractive atmosphere. Herschel's observation of a remarkable retardation of the occultations of Mimas and Enceladus has not been confirmed; but, perhaps, this is hardly to be wondered at. There are not ten telescopes in the world capable of following these two satellites up to the moment of their disappearance, and since the date of Herschel's observation there have only been five intervals, each of a few weeks only, during which the observation has been possible. It is true that where the farther part of the ring appears to meet the disc at an acute angle, its outline should be distorted if the planet has a refractive atmosphere; but it is easy to see that the distortion would scarcely be appreciable even with the most powerful instruments, and Mr. Grover's observation above recorded is sufficient to show that details observable with very moderate powers may for a long time escape observation. It will be presently seen that I am not here losing sight of the evidence pointing to a reflective power possessed at times and in certain latitudes by Saturn's atmosphere.

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* Schröter estimated the height of the atmosphere of Ceres at 668 miles (!); a result which can hardly be considered as established.

Now it has been observed by Professor Challis,* that the atmospheres of planets must have certain definite limits, since 'the density continually decreasing with the height, a point must at length be reached at which the upward repulsive force of an atmospheric stratum is just equal to the force of gravity; in which case there can be no downward repulsive force, and therefore no further extension of the atmosphere.' And he considers that the effect of an atmosphere on our estimate of a planet's diameter will depend on 'the relation between the gradation of density of the atmospheric strata and the curvature of the globe.'

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* In an article on the 'Indications, by Phenomena, of Atmospheres to the Sun, Moon, and Planets,' *Notices of the Astronomical Society*, vol. xxiii., pp. 230–235.

This relation may be such that a ray would 'pass *through* the atmosphere in a course which grazes, or is a tangent to, the interior globe;' or else, that rays could not reach 'the surface of the globe in a direction making with it an angle less than' a certain limiting angle. In the first case the apparent diameter would 'most probably not be sensibly increased;' in the second, 'the increase would be the angle which the whole height of the atmosphere subtends at the earth.' Now it is clearly possible that the atmosphere of a planet might in general exhibit one of these relations, but for a season might present the other, either over the whole planet or in certain latitudes only. And we have distinct evidence that Saturn's atmosphere is of variable refractive power: for whereas in nearly all pictures of Saturn, and notably in Mr. De la Rue's admirable engravings, the disc is darker at the edges than at the centre, so that this may be supposed to be the general appearance of the planet, the contrary appearance was presented in one instance to Bond II., and in another to Chacornac. Now it is perfectly clear that, if for a season the atmosphere over certain latitudes assumed the second state described by Professor Challis, while the remaining part was in the former—which we may be perhaps justified in calling the *normal* state—an apparent irregularity of figure would result. The outline of the disc would correspond in the former latitude to the upper limits of the atmosphere, in the latter to the limit of the interior globe; and we may suppose that in the intermediate latitudes the outline would pass from one limit to the other by indefinite gradations. There are reasons also for supposing that the 'reflective state' would be more commonly assumed in Saturn's temperate zones than near his equator or poles. For the causes to which our trade-winds are due are exaggerated in the case of Saturn. Now Sir J. Herschel thus describes the circulation of our atmosphere:—'In each hemisphere inferior currents of air run in on both sides towards the equator, and superior ones set outwards, all around the globe, from the equator towards the poles.' It is quite clear that neither near the poles nor near the equator, from or towards which these opposite currents tend, is it likely that there will be that tendency to stratification of the atmosphere into layers of variable density which favours the 'reflective state.' On the other hand, this state may be expected to occur—not commonly, indeed, but occasionally —in the temperate zones, where these currents attain their greatest velocity and steadiness of motion.

It need scarcely be remarked that an apparent difference of level of 500 or 600 miles in latitude 40° or 45°, would be fully sufficient to account for Saturn's 'square-shouldered' figure; but there are reasons for supposing that the height of Saturn's atmosphere in these latitudes exceeds the mean height. For it is found that barometric pressure attains its greatest value in the temperate zones; and although this phenomenon has never been fully accounted for, it appears highly probable that it is due to the rotation and figure of the earth; and therefore it seems probable that, in a planet of the figure, dimensions, and rapid axial rotation of Saturn, the excess of atmospheric pressure in the temperate zones would be still more marked.

It would be interesting to examine whether the square-shouldered figure seems to be connected with the occurrence of changes in the configuration of the Saturnian belts, or whether it is only assumed when the belts have remained for a long time in the same, or nearly the same position.

Intellectual Observer for August 1866.

THE PLANET SATURN.

The planet Saturn now (1870) presents his most interesting aspect. As he sweeps around his widely-extended orbit, occupying nearly thirty years in circling once around the sun, that mysterious ring-system which distinguishes him from all the orbs of heaven twice attains its widest opening. Fifteen years ago the southern surface of the rings was so much tilted towards the earth that its farthest part could be seen above the globe of Saturn. Then gradually as Saturn swept onwards towards the equinoctial points of his orbit, the rings became more and more foreshortened, until in 1862 their edge was turned towards us. After that the northern face became visible; and during all the years which have elapsed since 1862 this face has become more and more fully turned towards us, until now, as in 1856, the outline of the planet's globe lies wholly within the outline of the ring-system's outer boundary.

It was while the southern surface of the ring-system was turned as fully towards us as now the northern surface is, that the dusky, slate-tinted inner ring was discovered independently by Bond in America and Dawes in England. At that time, too, the signs of divisions in the ring-system were clearly recognised by many observers. It may well be that the present wide opening of the ring-system will be studied with scarcely less interesting results by those observers who possess adequate telescopic appliances; though, on the other hand, it is far from improbable that the low altitude which the planet now attains above the horizon will deter observers in our northern latitudes from studying Saturn so attentively as they otherwise would. Be this as it may, the present aspect of the planet is full of interest to the thoughtful. Much has been learned respecting Saturn during the last twenty years, and there can be little doubt that, independently of fresh discoveries, we may find much to reward us in the careful consideration of what has been so recently brought to our knowledge.

And here I may be permitted to remark, in passing, that it sometimes seems to me as though the astronomers of our day were apt to let the full significance of observed facts escape their notice. In the continual search for fresh knowledge, that which has been already obtained is sometimes neglected. Our observers are so industrious and skilful that new facts are being accumulated with unexampled rapidity. But it is getting a little out of fashion in the present day to dwell thoughtfully on past observations, insomuch that I feel it almost necessary to apologise for inviting attention to observations which were made many years since.

Yet to anyone who thoroughly grasps what astronomy teaches us about the ringed planet, how impossible it seems to exhaust the subject by any amount of study. That wonderful orb, circled about by the mighty mechanism of the ring-system, and the centre of a scheme of dependent globes equalling in number the primary planets of the solar system, may worthily employ many hours and days, nay, many months and years, of thoughtful study. The more we consider the subject, indeed, the more amazing and inexplicable the economy of Saturn's system seems to become. I can, at least for my own part, assert that I have never directed my thoughts afresh to the relations he presents without some hitherto unnoticed peculiarity attracting my attention. I propose now to touch on one or two points which have not yet, so far as I know, been dealt with by astronomers, and which seem to throw light on the physical constitution of this mighty orb and his fellow giants, Jupiter, Uranus, and Neptune.

Regarding Saturn either with a powerful microscope, or as presented in the admirable drawings recently taken by De la Rue, Browning, and others, it seems natural to inquire what signs the planet's disc presents of those peculiarities which would characterise our own earth, could we see it from Venus or Mercury with suitable telescopic power. Setting on one side for the moment the division of the earth's surface into large tracts of land and water, there are two most important relations which could hardly fail to be distinctly recognisable—I refer to the progress first of the *day*, and secondly of the *year*. To the astronomer, contemplating our earth from Venus or Mercury, it would be no difficult task to trace certain changes characterising the advance of day and the coming on of evening, in certain parts of the earth at least; while in a yet more distinct manner, supposing him to watch our earth, day after day, through the entire circle of the year, he would recognise the effects of the alternation from summer to winter in either hemisphere. If Saturn resemble our earth in having the sun as the chief ruler of his days and seasons, we may look in his case, also, for some traces of similar relations.

Let us now carefully consider what we might expect to find, and then inquire what the telescope actually reveals to us.

As regards the progress of day upon the earth, a distinction must be drawn between the temperate regions and the torrid zone. Undoubtedly even in our own latitudes we may recognise day after day in summer, often for weeks together, the formation of clouds during the morning hours, their gradual increase up to a certain hour, and their subsidence (accompanied by a change in their form and structure) towards evening. Supposing for a moment that this took place at all stations in our latitude, then our imagined astronomer in Venus or Mercury would recognise in that latitude-zone corresponding peculiarities. Close by the edge of the disc towards the west, he would be able to see the actual surface of the earth in those latitudes; the sky being still clear during the early morning hours in progress there. Casting his eye along the zone towards the east, he would find the zone grow whiter and whiter up to a part somewhat to the east of the middle point. This whitest part would correspond to the region where clouds were most numerous. Farther east the zone would still be whitish, and that to the very edge, since the clouds raised in the daytime (during such weather as we have been considering) do not disappear before sunset, but sink down like a pall upon the earth.

But, as I have said, it is not in temperate regions that the most marked diurnal changes are recognised. Let us consider the ordinary peculiarities of the equatorial day, or rather of the day in those regions of the earth where the sun passes almost to the zenith (the point vertically overhead) at noon-day. This is the region of greatest heat, and north and south of it lies the region of the trade-winds. Now let us hear what meteorologists have to say respecting the condition of the atmosphere, as regards the presence or absence of clouds during the day, in this region. 'In all places where the trade-wind blows constantly,' Kaemtz remarks, 'it does not rain; the sky is always serene; but it often rains in the region of calms. The ascending current (caused by the intense heat here) draws with it a mass of vapours, which condense as soon as they arrive at the line of junction between the upper and the lower trade-winds.

The sun almost always rises in a clear sky: towards midday isolated clouds appear, which pour out prodigious quantities of rain. These showers are accompanied by violent gales. *Towards evening the clouds dissipate, and when the sun sets the sky is perfectly clear.*' Buchan, in his excellent 'Handibook of Meteorology,' similarly describes the progress of the weather changes during a day in the calm regions. He adds that the daily rains of the belt of calms are to some extent analogous in their origin and causes to the formation of the cumulus cloud of temperate climates.

Now let us particularly note the position of a place where one of these diurnal rain-storms is commencing. Up to midday the sky has been relatively clear; the sun has passed nearly to the point overhead before the clouds gather, and he is actually overhead at noon when the whole sky is covered with black clouds. So that if anyone could take up a station where the illuminated hemisphere of the earth at that moment was fully turned towards him, the very centre of that disc would be the place where this state of things prevails. There, then, he would see the bright light indicating that the spot was cloud-covered—he would see 'the silver lining' of the black clouds which at the moment are pouring down their contents upon the portion of the earth concealed from him. But now let us suppose that he had watched this region of the earth from the early morning hours until it thus became concealed by clouds. It would come into view on the western side of the disc, and then travel across the disc (either in a straight line or along a curved path, according to the season) until it reached the centre. All this time it would grow less and less distinct, and when actually at the centre would be lost to sight under heavy clouds. But, still following its course towards the eastern side of the disc, our imaginary observer would see the bright light from the clouds grow fainter and fainter until, some time before reaching the edge of the disc, the region of the earth he had watched at first would reappear, —for we have seen that the skies clear up towards evening.

But what is true of one spot in this latitude is true of others. Every spot coming into view in the west would be clear of clouds, every spot crossing the middle of the disc would be hidden, and, finally, every spot passing off the disc on the east would be clear again. It is perfectly obvious, then, that the zone along which the spots lie would always present to our observer the same general aspect. This terrestrial zone of calms, which has been compared with the equatorial bright belt of Saturn, would appear to the observer dusky towards the west, where the earth's duller hues are seen through it; bright in the middle, where clouds reflecting white light are gathered over it; and towards the east of the disc the brightness would gradually diminish, until close by the eastern edge the dusky light seen in the western half would reappear. These peculiarities of appearance would be rendered all the more marked by the circumstance that the central part of the disc is illuminated more brightly by the sun than the parts near the edge.

We turn now to Saturn and inquire whether his equatorial bright zone presents these peculiarities. We might expect that a zone so bright and conspicuous as to be visible in a telescope of tolerable power—that is a telescope such as would be found in any well-appointed observatory—would exhibit some such characteristics as have been described. Assuming this belt to consist of sun-raised clouds, we might fairly look for signs of the progress of the Saturnian day, for the characteristics, in fact, of the morning, noon, and evening sky of the Saturnians. Nay, remembering how rapidly Saturn rotates, we might expect to find a more marked difference between the morning and the afternoon portions of his cloud-zone; since a part of the planet's surface sweeps through a more considerable portion of its daily circuit in any given period, than a corresponding part of the earth's surface.

We may well be surprised, then, to learn that the great equatorial bright belt of Saturn is absolutely uniform in light and texture except in parts so close by the edge of the disc that a difference of aspect is obviously referable to foreshortening alone. Not the slightest trace has ever been discerned of any peculiarities indicating the aggregation of clouds over the equatorial zone of the planet as the Saturnian day progresses!

It would almost seem to follow from this fact alone that the Saturnian cloud-belts are not raised by the sun's action.

Let us inquire, however, whether seasonal changes are more marked than diurnal ones. Since the Saturnian year lasts for about twenty-nine of our terrestrial years, it should follow that seasonal changes would proceed much more steadily and certainly. We have to consider what those changes would be in the case of our earth, and then to inquire whether any corresponding variations are discernible in the aspect of Saturn.

Again, I prefer to limit the consideration of annual processes of change to the tropical regions, where a regularity of variation prevails which is wanting in the temperate zones. It is further convenient to consider these regions because we have already examined one marked peculiarity of the tropical day, and shall thus be prepared to deal with a closely related peculiarity of the tropical year.

We have seen that a heavy daily rainfall takes place in that particular latitude on our earth where the sun is overhead at noon. Now the position of this latitude obviously changes during the course of the year. In spring the equator is the region of greatest midday heat. After spring the latitude of greatest heat approaches us, and at midsummer the sun is vertical at noon in all places lying 23½ degrees north of the equator. After midsummer, the region of greatest midday heat withdraws from us, and at the autumnal equinox it again coincides with the equator. After autumn the latitude of greatest heat passes south of the equator, reaching its greatest southerly digression at midwinter. And finally after midwinter the region of greatest midday heat returns to the equator, which it reaches at the vernal equinox.

But we must assure ourselves that the weather changes correspond to these relations; for it might be that the existence of a calm zone was a peculiarity not wholly depending on the position of the midday sun. I might quote numerous authorities to show how the zone of calms in reality follows the sun, but will limit myself to two. Buchan, to whom I have already referred (as regards the progress of the diurnal changes in the calm zone), writes thus respecting the nature of that zone and the annual changes in its position:—

'The region of calms is a belt of about 4° or 5° in breadth, stretching across the Atlantic and the Pacific, generally parallel to the equator. It is marked by a lower atmospheric pressure than obtains to the north and to the south of it in the regions traversed by the trade-winds. It is also characterised by the daily occurrence of heavy rains and severe thunderstorms. The position of the calms varies with the sun, reaching its most northern limit (25° north latitude) in July, and its most southern (25° south latitude) in January.'

The other passage I propose to quote is from Captain Maury's charming work, the 'Physical Geography of the Sea.'

The passage is interesting as indicating the office which the calm zone seems to fulfil in the economy of the earth.

'After having crossed the cloud-ring (says Maury) the attentive navigator may perceive how this belt of clouds, by screening those parallels over which he may have found it to hang, from the sun's rays, not only promotes the precipitation which takes place within these parallels at certain periods, but how also the rains are made to change the places on which they are to fall; and how by travelling with the calm belt of the equator up and down the earth this cloud-ring shifts the surface from which the heating rays of the sun are to be excluded; and how by this operation tone is given to the atmospherical circulation of the world, and vigour to its vegetation. Having travelled with the calm belt to the north or south, the cloud-ring leaves a clear sky about the equator; the rays of the torrid sun then pour down upon the solid crust of the earth there, and raise its temperature to a scorching heat. The atmosphere dances, and the air is seen trembling in ascending and descending columns, with busy eagerness to conduct the heat off and deliver it to the regions aloft, where it is required to give dynamical force to the air in its general channels of circulation. The dry season continues; the sun is vertical; and, finally, the earth becomes parched and dry; the heat accumulates faster than the air can carry it away; the plants begin to wither, and the animals to perish. Then comes the mitigating cloud-ring. The burning rays of the sun are intercepted by it; the place for the absorption and reflection and the delivery to the atmosphere of the solar heat is changed; it is transferred from the upper surface of the earth to the upper surface of the clouds.'

This series of changes is not only most important to the inhabitants of the earth, but it is of such a character that any observer able to watch the earth throughout the whole course of a year, as we watch the planet Saturn, could not fail to become readily cognisant of it. The actual range over which the central line of the calm zone oscillates northwards and southwards is forty-seven degrees (Buchan's numbers referring to the extreme northerly and southerly limits of the zone). Now if a globe be placed at some considerable distance from the eye, and an arc of forty-seven degrees marked on the globe is so placed that its middle point seems to occupy the middle of the disc presented by the globe, then the apparent length of the arc will be as nearly as possible two-fifths of the globe's diameter; so that the actual range of the calm zone viewed as we have imagined would correspond to no inconsiderable portion of the earth's apparent diameter. Only it is necessary to remember that if our observer always viewed the earth so as to see the whole of her illuminated hemisphere, then the calm zone would always cross the centre of the disc. Near either equinox, it would appear as a straight line across the centre. In July it would appear as somewhat more than half an ellipse, the two ends bowed upwards, and the middle point of the arc (which would correspond to an extremity of the shorter axis of the ellipse) coinciding with the centre of the disc. In January the calm zone would have the same figure as in July, only the two ends of the elliptic arc would be turned downwards. The curvature of the arc would be, for the reasons above alleged, most obvious; in fact the lesser axis of the complete ellipse would be two-fifths of the greater.

Applying these considerations to the case of Saturn, on the supposition that his equatorial bright belt corresponds to the calm zone of the earth, we may expect to find an even more marked change of appearance in this belt than we have inferred in the case of the earth's calm zone. For the inclination of the earth's equator-plane to the path in which she travels is but twenty-three and a half degrees; the corresponding inclination in the case of Saturn is nearly twenty-seven degrees. It will obviously be so much the easier to infer whether the belt exhibits those peculiarities which correspond to the theory that it is due to solar influences.

Now the bright belt on Saturn *does* change in its apparent shape (precisely as the Saturnian rings do) in the course of a Saturnian year. At the present time, for instance, the bright belt, seen in an ordinary astronomical (inverting) telescope, is bowed very obviously, with its convexity upwards. But instead of the central line of the belt passing across the centre of Saturn's disc, it has precisely the position which Saturn's equator, if marked as a line upon the surface of the planet, would seem to occupy. In other words, the central line forms a half ellipse, the middle of whose greater axis occupies the centre of Saturn's disc, instead of the extremity of the lesser axis being at that point. The bright belt is in fact, as its name implies, equatorial, *now*, during the summer of Saturn's northern hemisphere; whereas the calm zone of the earth at the corresponding season is not equatorial, but coincides with the Tropic of Cancer.

Here again, then, we have very clear and positive evidence against the theory that *this* Saturnian belt at any rate is due to solar action.

It is also worthy of remark that the evidence is not affected whatever opinion we may form as to the general uniformity or diversity of the surface of Saturn. If the surface of Saturn be diversified, then the constancy and uniformity of the equatorial belt become so much the more surprising; if, on the other hand, the surface of Saturn is very uniform, then those seasonal changes which we have considered ought to proceed so much the more regularly. On the earth they are interrupted, as we know, in certain places, owing to the configuration of oceans and continents;

and monsoon weather-changes replace the systematic progression observed elsewhere. But the very uniformity of the bright belt on Saturn forbids us to regard such peculiarities as available to aid us in interpreting the phenomena we have been considering.

It is further noteworthy that an objection which might have been made to the argument founded on the diurnal constancy of the Saturnian equatorial belt, is not available against the argument just dealt with. Saturn is so much farther from the sun than the earth is, that a certain sluggishness might be supposed to characterise processes depending upon the sun's action; and therefore it might be supposed that a cloud-belt, once formed by the sun, would be carried round by Saturn's rapid rotation without being dissipated or in any way modified, whether night or day prevailed on Saturn. But in the case of the seasonal changes we have been considering, no such argument can be admitted; for whatever view we might form as to the possible constancy of a cloud-belt during the ten hours of the Saturnian day, it would clearly be unreasonable to infer that the seven-yearly seasons (or quarters) of Saturn would be too short to produce their due effect on the position of the great cloud-zone. If the sun during his slow passage northwards and southwards from the celestial equator of Saturn cannot modify the position of the cloud-zone, it seems altogether incredible that his action can have been in any way concerned in the formation of that zone.

Yet further, it is wholly impossible for any thoughtful student of the Saturnian belts to suppose that the action to which they are due is of so inert and sluggish a nature as would be implied by the supposition just referred to. The changes which take place in the figure and position of the dark belts lying on either side of the equatorial bright belt, are sometimes singularly rapid, especially when account is taken of the enormous extent of surface belonging even to the least of these belts.

For my own part, I confess I cannot but regard these facts as affording very strong evidence in favour of a theory to which I had been led by other considerations. If the sun is not the agent in producing those cloud-masses which constitute, we may assume, the bright belts of Saturn, we must look for the real origin of the belts in some action exerted by the planet's own mass. In other words, we seem led to the consideration that the mass of Saturn is sufficiently heated to cause currents of vapour to rise continually from his surface to be condensed into the form of cloud when they reach the upper regions of his atmosphere. Why such processes should take place in certain regions rather than in others, it would perhaps be difficult to determine. We know so little at present of the extent, constitution, and condition of the atmosphere of Saturn, that it is difficult to reason as to processes of change, excited by heat whose seat lies perhaps hundreds of miles beneath the surface visible to us. It may be remarked, however, that a similar peculiarity exists in the case of the sun. Indeed, a somewhat surprising resemblance exists between Saturn and the sun, as regards many important characteristics. The planet, like the sun, is of low specific gravity—very far lower than the earth's; as the sun has eight primary attendants, so Saturn has eight satellites; and as the sun has his attendant disc of minute bodies (seen in the zodiacal light), so Saturn has his ring system, composed, in all probability, of multitudes of minute satellites travelling in independent orbits around him.* Is it not possible that the relation necessary to make the analogy complete may be actually fulfilled, and that Saturn is a source when heat is supplied to the orbs which circle around him? We have seen that reasons exist for regarding the Saturnian belts as resulting from processes excited by the planet's internal heat; and we are thus prepared to regard less suspiciously than we might otherwise have been disposed to do, any evidence tending to show that such processes are of a very remarkable character. The same forces which can generate belts covering a surface many times exceeding the whole surface of our earth in extent, may also, it is conceivable, produce other effects clearly recognisable from our distant station.

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* The theory that Saturn's rings are thus constituted has been so commonly attributed to myself of late years that I feel bound to take every opportunity of disclaiming all credit whatsoever in the matter. I hold that it has been put beyond question that the Saturnian rings are neither formed of a continuous solid nor of a continuous fluid substance, and also that they are not wholly vaporous. But I have had no part in establishing this result, which is due

It is perhaps only after preliminary evidence of this sort has been adduced, that most astronomers would be ready to listen even for a moment to such arguments as I have adduced in my treatise on 'Other Worlds than Ours,' to show that the apparent outline of Saturn is liable to change. Notwithstanding the wonderful caution with which Sir William Herschel's observations were carried on, his unwillingness to accept conclusions even after a long series of apparently convincing researches, and the clear-sightedness with which he *reasoned out* the interpretation of his observations, astronomers had agreed to reject (*as resulting from illusion*) the views which he formed respecting the 'square-shouldered aspect' of Saturn. Bessel's exquisite measurements of the planet's disc seemed to show convincingly that

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solely to the labours of Bond, Pierce, and Maxwell. I have presented some of their reasoning in a popular form in my treatise on Saturn, but it is distinctly presented as their reasoning, not mine. One or two considerations helping to make the evidence more convincing perhaps to the general reader are due to me; and in particular the argument founded on the dusky spaces seen by Bond on the great middle ring. But though this last argument affords in itself a demonstration that we here see *through* this apparently continuous ring, I can take no credit whatever for demonstrating what had already been established by the arguments of others.

it is not 'square-shouldered,' but truly elliptical, insomuch that, as Professor Grant remarks, 'no doubt could henceforth exist that the figure of the planet is that of an oblate spheroid. . . . It is impossible,' he adds, 'to contemplate Bessel's numbers (as compared with what theory required) without a feeling of admiration of the theory which is capable of responding so faithfully to the requirements of nature, and of the exquisite skill displayed by the illustrious astronomer who executed measures so singularly delicate as those above given, with a success apparently so complete.'

Yet, while fully admitting the justice of these remarks, I have long felt that Sir William Herschel's observations of Saturn's figure are not to be so summarily dismissed. To quote words which I wrote five years ago, the astronomer who 'examined Saturn's ring for ten years before he would accept the theory of its being divided, and watched a satellite for two years before he would pronounce an opinion on its rotation,' was not the man to be misled by illusions, or to make confident statements without adequate reason. A 'suspicion' of either Sir William Herschel's or Sir John's would counterbalance with me the most positive assertions of ordinary astronomers. But in this case it was no suspicion. Let us hear what Herschel himself says, and we shall be in a position to determine whether it is likely that this eminent observer was deceived by a mere illusion, and that too when he was in the very zenith of his career as an observer. 'In order to have the testimony of all my instruments on the subject of the structure of the planet Saturn,' he writes, referring to the observations made in May 1805, 'I had prepared the 40-feet reflector for observing it in the meridian. I used a magnifying power of 360, and saw its form exactly as I had seen it in the 10- and 20-feet instruments. The planet is flattened at the poles, but the spheroid which would arise from this flattening is modified by some other cause, which I suppose to be the flattening of the ring. It resembles a parallelogram, one side whereof is [parallel to] the equatorial, the other [to] the polar diameter, with the four corners rounded off so as to leave both the equatorial and the polar regions flatter than they would be in a regular spheroidal figure.' He determined by actual measurement the position of the protuberant portions which formed the corners of this 'square-shouldered' figure, and placed them in latitude 43⅛° north and south of the equator. He measured the amount of the protuberance, making the polar, equatorial, and maximum diameters as 32, 35·4, and 36. He renewed his observations in 1806 with the same result. But what is most remarkable of all, he observed in 1807 that a change had taken place in the aspect of the planet, the two polar regions now presenting a different shape, the northern regions being most flattened, the southern 'curved or bulged outwards.' Admiral Smyth remarks that 'this singularity was verified by the younger Herschel on June 16 of the year 1807; and is, I believe, his first recorded astronomical effort.'

When to the above evidence is added all the evidence recorded in my 'Other Worlds'; the fact that such observers as Bond and Airy, using such instruments as the Harvard refractor (perhaps the finest in the world) and the refractor of the Greenwich Observatory, have noticed similar appearances; and that other practised observers less known to fame confirm their observations—we can no longer, surely, class the 'square-shouldered aspect' of Saturn among the 'myths of an uncritical period.' *

Now, assuming that Saturn is liable to occasional changes

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* Let me note further that Sir William Herschel's measurement of the compression of Saturn in 1789 'has been found,' Professor Grant tells us, 'to accord exactly with that derived from the most recent micrometrical measures of the axes of the planet.'

of figure—for undoubtedly his ordinary figure is that of an oblate spheroid—we have evidence of the existence of forces of the most amazing character beneath the seemingly quiescent zones which we have been accustomed to regard as the true surface of the ringed planet. We may be doubtful whether they be forces of upheaval, or whether an intense heat loads the atmosphere of Saturn from time to time (in the particular latitudes which seem to bulge outwards so strangely) with enormous quantities of vapour, to be condensed at an exceptionally high level; or whether the sudden dissipation of cloud-masses existing in other latitudes causes these peculiarities of appearance. But it is in any case most certain than an energy—a vitality so to speak—exists out yonder, which we have hitherto been far from associating with this distant and dimly lighted world. No moderate processes of change would suffice to cause the figure of a planet to vary appreciably when observed from a distance of some nine hundred millions of miles. As seen from the satellites, the farthest of which is but a million and a quarter of miles from Saturn, the planet must appear the scene of a wondrous turmoil. It is probable, indeed, that the true substance of the planet, which may be, for aught we know, absolutely incandescent through the intensity of its heat, is always veiled, even from these relatively near regions, by the masses of vapour continually thrown off to condense into cloud-strata at higher or lower levels. But the evidences of intense action can hardly fail to be perfectly obvious, even though the actual source of such action is concealed from view.

Let me remark, in conclusion, that the theory here put forward is not urged from any desire to exhibit novel or startling views, but as serving to explain, better than any other theory I can imagine, a series of observed facts which cannot judiciously be neglected or forgotten. I have preferred to give no consideration whatever to 'the question whether the larger planets have or have not as yet cooled down, by radiation, to a sort of normal temperature,' because in the present state of our knowledge that question is purely speculative. My theory is directed to explain observed facts: if it happens to throw some light on the question of the original formation of various members of the solar system, that is merely by the way; the theory must stand or fall according as it can be shown to be in agreement or not with past and future observations.

Fraser's Magazine for September 1870.

NOTE.—The following papers are taken from *The Intellectual Observer and Student* for October 1867, November 1868, and October 1869. I believe that by thus presenting the records of successive years in company with the anticipations formed each year, the interest of the subject is enhanced. Indeed, one of the most remarkable facts lately learned respecting the November meteors is the circumstance, that for so many years in succession—1866, 1867, 1868, 1869, and 1870—there have been important displays. In 1871 none but a few stragglers of the November system were seen; and it may now be assumed that no shower of November meteors need be looked for until the year 1899.

THE NOVEMBER SHOOTING STARS.

I.

IT is probable that there will be this year [1867] an exhibition of the November shooting stars, though it is uncertain whether the phenomenon will be so well seen in Europe as it was last year. As a *display* the shower is not likely to be so splendid as it was in 1866, since on November 14th of the present year the moon will be nearly full. However, there can be no doubt that the November meteors will be looked for again with great interest, since the discoveries which have been made respecting the orbit in which they move have presented them to us in a new aspect.

When the shower of November last was under discussion, it was very noteworthy how indistinct were the views of many persons—I may even say of many *astronomers*—respecting the relations of the earth's globe, as it travelled onwards rotating in its orbit, to the meteor stream which it encountered. I do not here refer to the doubt and obscurity under which the question of the path actually pursued by the meteors rested at that time. The investigation of this question was one of extreme difficulty, one which taxed—and not lightly—the powers of the highest modes of mathematical analysis. But many appeared to find considerable difficulty, or failed altogether, in forming an estimate of the circumstances under which the meteors became visible to us. The existence of a 'radiant point' from which all the shooting stars appeared to travel, in whatever part of the sky they made their appearance, was a phenomenon which—although in reality it inferred the solution of the problem of the meteors' origin—yet presented difficulties to many observers. The questions that were asked and the suggestions that were offered on this and kindred points, were many and amusing. One observer, noticing the comparative absence of meteors from the immediate neighbourhood of the 'radiant point,' suggested in explanation of the peculiarity, that the earth was passing through a sort of tunnel traversing a bed of meteors; thus in the path along which the earth travelled there were no meteors or few—previous passages along the same track having cleared the way—but many meteors grazed the earth's atmosphere, the bore of the tunnel only allowing the solid globe of the earth to pass freely. And, indeed, the supposition that shooting stars are only seen when *grazing* our atmosphere has been commonly entertained and expressed even by astronomers of eminence. Sir John Herschel, for example, speaks of meteors as 'bodies extraneous to our planet, which only become visible when in the act of grazing our atmosphere.' The idea, however, is erroneous, as we shall presently see. Another remarkable question which was asked soon after the occurrence of the November shower, served still more clearly to exhibit the indistinctness of the views commonly held; meteors having been seen at Cape Town at the same hour (actual time) as in England, it was asked how the same meteors could be seen in both places, unless they had travelled as satellites round the earth? A well-known chemist, who has lately published a work on meteors, speaks of the received opinion of the cosmical origin of meteors as, after all, merely conjectural, and he evidently leans towards the theory that they are satellites of the earth. Lastly, in Guillemin's 'Heavens,' a view is expressed (and illustrated by an elaborate figure), which is wholly inconsistent with observed appearances;—the notion, namely, that a single stream of bodies could give rise to both the November and August showers.

It is evident, therefore, that there is room for a careful examination of the actual state of things during the occurrence of the November shower. By considering the position of England on the rotating earth, during the time of the display, we shall be able to form clear views on this point.

I must first, however, mention briefly the true meaning of the existence of a 'radiant point.' Once this phenomenon is established, *all doubt whatever* respecting the cosmical origin of a shooting-star shower disappears. It is not true that the theory of a cosmical origin is now a conjectural one; it is established on a thoroughly firm basis. The phenomenon of a radiant point proves in fact *this*, that the paths in which the meteors intersect our atmosphere are all parallel *in space* throughout the time that the shower is visible. Now the display lasting several hours, during which the earth moves through a large angle round her axis of rotation, it is quite clear that the display cannot have a terrestrial origin, since if it had, the direction of the shooting stars might be *expected* to change *correspondingly*, and *would certainly* not change after so artificial a manner that for several places at once the effects of the earth's rotation would be *exactly compensated.* An equatorial telescope, for instance, is made by clockwork always to point to the same star; but we know that no telescope poised *at random* and moved at a *random rate* would do so. Just, therefore, as a person seeing the same star for a considerable time through the tube of a telescope, knows certainly that he is looking through an equatorial rendered artificially independent of the earth's rotation—so, seeing shooting stars moving always from a fixed point among the stars, we know for certain that the direction of their motion is independent of the earth's rotation, and therefore—there being no possibility of an artificial arrangement corresponding to that of the equatorial—that the shooting stars come from external space. The notion of a lunar origin, and the satellite theory of meteors, are similarly overthrown, though indeed, at the present day, no competent person entertains either of these views, which are, for other reasons, wholly untenable. When the occurrence of a 'radiant point' is coupled with 'annual periodicity and independence of geographical position, referring us at once to the place occupied by the earth in its annual orbit,' the most sceptical (or, in this case, we must say those least able to appreciate the mathematical demonstration of the meaning of a radiant point), must be led 'directly to the conclusion that the earth is liable to encounters or concurrences with meteor streams in their progress of circulation round the sun.'

It must be mentioned that the earth's motions have their effects upon the apparent motion of bodies moving in space. The motion of rotation, however, may be neglected in comparison with the motion of revolution and the proper motion of meteoric bodies. Travelling in space, under the sun's attraction, it is unlikely that, at the moment of encountering the earth, they have a less velocity than that due to a body moving circularly round the sun at the earth's distance (a rate very slightly less than the earth's) and they may have a velocity nearly half as great again as this. Between these values their velocity may be assumed to lie. Further, their velocity, relatively to the earth, must lie somewhere in value between the sum and difference of their actual velocity and the earth's, or between zero and about forty-five miles per hour; the first value giving the extreme case of meteors travelling in the same direction, and at the same rate as the earth; the second giving the case of meteors travelling in a parabolic orbit, and encountering the earth *directly*, just when they are *in perihelion*.

I have mentioned these limits and considered the nature of meteoric motion relatively to our earth, because it is on this relative motion that the position of the 'radiant point' depends. If we suppose the earth reduced to rest, and her motion, *reversed*, added to the motion of the meteoric stream, we get the same *relative* motion and the same *radiant point* as under the actual circumstances of the case. For clearness of explanation let us suppose this to happen, and that on the night of November 13–14 the earth's motion of revolution is non-existent (her motion of rotation continuing, however), and that the meteors are sweeping towards her from their radiant point (i.e. at a rate and in a direction resulting from the combination of their own actual motion, and the earth's motion applied in a reversed direction).

These suppositions being made, we can have no difficulty in selecting a suitable point of space from which in imagination to view our earth. The 'radiant point' is clearly the proper point to select. If the reader, therefore, will suppose himself somewhere in space, between ε and μ Leonis, and armed with sufficient optical power, he will be prepared for the examination of the illustrative Figs. 4 and 5. In these the earth is supposed to be viewed from such a direction; in Fig. 4, at about a quarter-past twelve, and in Fig. 5, at about a quarter-past two, Greenwich solar time, on November 14th, in any year. The shaded half of each hemisphere is the portion turned from the sun, the apparent boundary of this portion being a straight line, because the radiant point (as respects its *longitude*) is situated very nearly in the direction towards which the earth is moving at the time. But since the radiant (as respects *latitude*) was raised some 10° to the north of the ecliptic, the north pole of the earth is brought more into view than it would be to an observer placed at a point towards which the earth is actually moving at the time. In fact, the presentation of the earth towards the radiant point happens to be almost exactly the same (as to inclination of the polar axis) as the presentation of the earth towards the sun at the time of summer solstice. Without entering further into these points, it will suffice to say that Figs. 4 and 5 are the results of mathematical calculation and careful construction—not imaginary figures set down partly at random, as is too often

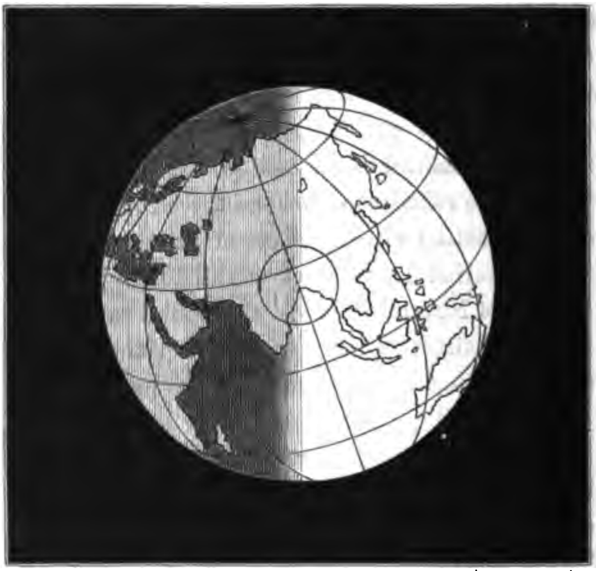

FIG. 4.—The earth as supposed to be seen from the 'radiant' of the November meteors, at 12h. 15m. (night).

the case with illustrations of this sort. I am particular to mention this, because when it is known that an astronomical picture represents actual facts, as closely as possible, the student will undertake the study of the picture with some hope of information and instruction, whereas the study of illustrations (so-called) not carefully constructed—and nine out of ten figures in our works on popular astronomy fall under this category—is often worse than unprofitable.

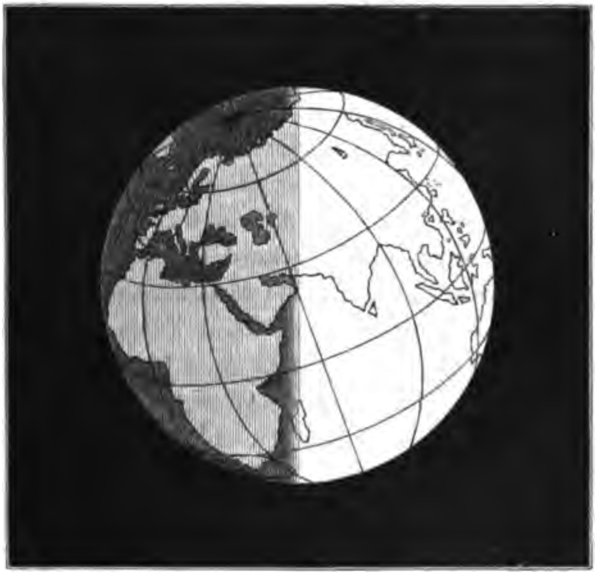

FIG. 5.—The earth as supposed to be seen from the 'radiant' of the November meteors, at 2h. 15m. A.M.

Around London and Calcutta, in Fig. 4, and around London and Cape Town, in Fig. 5, oval and circular spaces are indicated. It is necessary to explain the meaning of these. Assuming the depth of our atmosphere to be about seventy miles—or, at any rate, that meteors are not commonly visible at greater heights—it is easily shown that the segment of atmosphere cut off by a plane touching the earth at any point, has a circular base about 1,500 miles in diameter. Thus neglecting the effects of refraction which would slightly increase the dimensions of the segment, we have this result, that no meteor can be seen from any point of the earth's surface further than 750 miles from the point over which such meteor is vertical. We have very strong evidence, showing that 70 miles is about the height at which meteors appear, the evidence of meteors appearing at a greater height being very doubtful. Hence, when a meteor is seen low down towards the horizon, it may be confidently assumed that the point over which this meteor is vertical lies within 750 miles of the place of observation. Now the ovals and circle in Figs. 4 and 5 mark the limits of the space over some point of which a meteor must be vertical to be seen from the centre of the space. For instance, a meteor appearing at a point vertical over Madrid, or Turin, or Berlin, or Stockholm, might *just* be visible from London, appearing just above the horizon; but a meteor vertical over Gibraltar, or Rome, or St. Petersburg, would not be visible in England.

Now, if we consider Fig. 4, we shall see that about two hours before the time indicated by that figure (a quarter-past twelve at night), London is just becoming visible on the edge of the earth's disc; but the edge of the oval space round London comes into view more than an hour earlier—that is, at about nine o'clock. This is the earliest hour at which a member of the November system can by any possibility be seen in London. Meteors seen at this hour would be momentarily visible in the eastern horizon, moving upwards. When London comes to the border of the visible hemisphere, meteors may be looked for over the whole space between the eastern horizon (that is from south, through east to north) and the zenith, travelling (more or less) upwards unless they appeared nearly towards the north or south, when their motion would be horizontal. When the whole of the London oval space is in view, meteors may be looked for over the whole heavens. A little consideration will show that at and after this time, conspicuous meteors will be seen more plentifully over the western half of the heavens. If the mere number of meteors indeed were alone considered, the contrary would be the case. But the paths of meteors being from a point east of London (it is clear that both in Fig. 4 and in Fig. 5 we are looking at London from the east), they would have in general an apparently westward motion, and all those having long visible tracks would be towards the west.

It is also evident from Figs. 4 and 5, that meteors increase in number (*cæteris paribus*) as England, through the earth's rotation, approaches the centre of the disc visible from the radiant point, or—which amounts to the same thing—as the radiant point rises above the horizon. It is clear, for instance, that the oval space round England in Fig. 5 is greater than the oval in Fig. 4; and that at an hour later than that indicated in Fig. 5, the oval is yet greater. The oval round England is greatest at about a quarter-past six, when the meridian of London is a diameter of the disc. The effects due to this cause of variation ought to be considered in estimating the actual changes in the richness of the shooting-star stream as the earth traverses different strata. For instance, the increase which actually occurred after midnight, last November, was partly due to this cause, while the diminution which took place subsequently to 1h. 30m. or 1h. 45m., was partly checked by this cause.

Let us stay for a moment to compare with the effects just considered, those occurring in other latitudes. It is clear from Figs. 4 and 5, that countries in northern latitudes are more favourably situated than countries in southern latitudes, as respects their chance of seeing the November star-shower. Thus, if we consider the short part of the arc traversed by Cape Town, which lies within the darkened part of the disc, it is clear that the *à priori* probability that observers there will see the phenomenon is small. The hour at which Cape Town reaches the diametral meridian being about 6h. 15m. Cape time, it is clear that the moment at which Cape Town enters on the part of the disc visible from the radiant point, is about 2h. 15m. The oval round Cape Town begins to enter this part of the disc rather more than one hour earlier. Thus, unless the phenomenon occurs between about one o'clock and daybreak (it will be seen that Cape Town enters the enlightened half-disc, or, in other words, *the sun rises* there, soon after five), it will not be seen at all at Cape Town; and that it should be well seen, it is necessary that the epoch of maximum richness should occur between 1h. 30m. and 3h. 30m. Cape time. It happened last November that the shower reached its maximum at 2h. A.M. Cape time, and was therefore well seen there.*

In tropical regions north of the equator, which enter on the hemisphere turned towards the radiant during the continuance of the shower, the display is likely to be grander than elsewhere, since the circular space around any point in such regions would be seen as an oval of much less eccentricity than that around places in high latitudes, during a part at least of their passage across the darkened part of the disc. At Calcutta, for instance, the boundary of visibility is appreciably circular (as shown in Fig. 4) a short time before sunrise. At this hour, last November, the shower had not reached its full splendour, and therefore the richer part of the display was not seen in Calcutta. In Nubia, Egypt, Asia Minor, and Greece, the shower was more favourably seen. Mr. Schmidt, for instance, reported a very rich display at Athens, reaching its maximum at 2h. 15m. local time, or about 12h. 45m. Greenwich time; very nearly the hour illustrated in Fig. 4. The display in India (at Kishnagur, fifty miles from Calcutta) began before four o'clock, and continued till daylight. At 4h. A.M., Calcutta mean time, which corresponds to 10 P.M. Greenwich time, London had not yet reached a position for a favourable view of the display.

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* For the same reason that meteors are more commonly seen in northern latitudes from July to December, they are more commonly seen in southern latitudes from January to June. An examination of Figs. 4 and 5 will illustrate the cause of this peculiarity, viz.:—the presentation of the northern and southern poles respectively towards the direction of the earth's motion. It is worthy of notice that Mr. Maclear records the observation of several meteors last November, before the hour at which Cape Town (or the space included within the oval in Fig. 5) entered on the hemisphere turned towards the radiant; or, in other words, before the radiant rose above the horizon: but none of these belonged to the November system, as was evinced by the direction of their motion.

It will be seen from Figs. 4 and 5, that during nearly the whole time that the display continued, last year, in England, every visible shooting star was travelling towards the earth's surface, *not* grazing the atmosphere. Thus no shooting star which fell within the oval line marked round England in Fig. 4, or in Fig. 5, could have failed to reach the earth's surface, unless dissipated in the upper regions of air. And, indeed, independently of the consideration of the November shower and its radiant, it is quite clear that of meteors which pass into our atmosphere, by far the larger number travel in a line which produced meets the earth's solid surface. For, in whatever direction a meteor stream is travelling, the earth, seen from the radiant point of the stream, must present an appearance corresponding to that illustrated in Figs. 4 and 5. The pole may be more or less bowed towards, or from, the direction in which the meteors are travelling (relatively) towards the earth, and other countries than those presented in the figures may be turned towards the meteor-flight; but a circular disc, apparently fringed with a comparatively narrow border of atmosphere, must in every case be presented towards the meteor-stream. Only those meteors which impinge on this fringe, a circular ring seventy miles wide,* can possibly free themselves by passing through (or grazing) the atmospheric envelope. All those meteors which are making for the apparent disc of the solid earth, a circle nearly 4,000 miles in radius, must inevitably reach the earth, either in a solid form or in the form of meteoric dust, after being dissipated in their passage through the upper atmospheric layers. Assuming that every meteor making for the fringe escapes, which is, however, utterly improbable, it may easily be calculated that for every meteor grazing our atmosphere (at a height not exceeding seventy miles), twenty-eight travel directly towards the earth's surface. But the proportion must in reality be very much greater, since our supposition implies the possibility of a meteor travelling through the air in a direction actually tangent to the earth's surface, or passing through about 1,450 miles of air, including the densest strata. Since meteors seldom penetrate to a vertical depth of more than twenty or thirty miles, without dissolution, it is very unlikely that meteors travelling parallel to the horizon should penetrate to a vertical depth even of ten or fifteen miles—since, to do so, their actual path through the air would be many times longer. Assuming that meteors could escape after penetrating in this manner to a depth of twenty miles, we should have, for every meteor so escaping, almost exactly one hundred whose substance, whole or dissolved, would reach the earth. Even escaping meteors would never again appear as members of the November shower, since their orbit, after grazing contact of the kind supposed, would be very different

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* Of course, not in reality such a ring, but apparently so, viewed from the radiant point of the meteor-flight; and intercepting the same proportion of meteors as if actually so.

(owing chiefly to their loss of velocity) from that which they originally pursued.

The fact that such multitudes of meteors have, during so many and such brilliant displays of November showers as have been recorded, been stolen by the earth from the stream to which they belonged, serves to afford some conception of the immense number of meteors forming the November stream. Yet clearer views will be formed on this point if we consider the evidence we have respecting the length, breadth, and thickness of that cluster, during the passage through which the display is visible. I have not space to dwell here on Adams's investigation of the meteoric orbit. But it is necessary to point out that we must now greatly increase our estimate of the length of the cluster causing the November showers. The recurrence of displays during two or three consecutive years was simply accounted for on the theory of a nearly circular orbit, without assuming for the cluster a length of more than a few millions of miles. Now that we know that the meteor-flight travels in an orbit of great eccentricity, and with a period of 33¼ years, we know that the portion passed through by the earth in one year is several hundreds of millions of miles away when the earth next passes through the meteor orbit. Hence the recurrence of displays leads us to estimate the length of the cluster by hundreds of millions of miles, instead of by mere millions.

Next, for the breadth of the stream. On this point we have no exact information. It is sometimes assumed that the fact that the display may be seen in one hemisphere, while in another it is not seen (as last year, for instance, in America), points to a limit of breadth. But this is not the case. If we consider Figs. 4 and 5 we shall see that America was on the sheltered side of the earth during the whole time of the display. When America had come to the side turned towards the radiant, the earth's globe had, in all probability, passed through the meteor-stream. So that the limits of the *thickness*, and not of the *breadth* of the stream, were indicated by the non-visibility of the meteors in America. Before the display had begun in England, the meteors were seen from Kishnagur, fifty miles north of Calcutta, and they continued visible until the time of sunrise there. This would assign a breadth of *not less* than 4,000 miles to the stream. But as, throughout the continuance of the display, the earth was crossing the breadth of the stream at the rate of about 1,000 miles an hour, we can assert positively that the breadth of the stream exceeded 6,000 miles. In reality, however, a very much greater breadth may be assigned, with great probability, to the meteor-stream. For if we consider the nature of the stream and the manner in which it has been probably generated in the track of Comet I., 1866, we shall see the great probability that its breadth exceeds its thickness. The causes tending to make meteors leave the mean plane of motion are much less efficient than those tending to distribute the meteors over that plane. Now the earth, during the time of the display, was crossing the *thickness* of the meteor-stream at the rate of about 18,000 miles an hour. Therefore, since the display lasted at least six hours (counting from the time of its being observed in India, when England was, as yet, on the earth's sheltered side), we cannot assign to the stream a less thickness than 100,000 miles. The breadth is probably at least ten times as great.

It may be assumed as certain that it is the passage of the earth through the *thickness* of the meteor-stream which limits the duration of the display.

I shall conclude by quoting two observations, showing that the fine powder in which meteors reach the earth may be detected. Dr. Reichenbach collected dust from the top of a high mountain, which had never been touched by spade or pickaxe; and on analysis he found this dust to consist of almost identically the same elements as those of which meteoric-stones are composed—nickel, cobalt, iron, and phosphorus. Again, Dr. Phipson notes that, 'when a glass, covered with pure glycerine, is exposed to a strong wind, late in November, it receives a certain number of *black angular particles*,' which 'can be dissolved in strong hydrochloric acid, and produce yellow chloride of *iron* upon the glass-plate.' I quote these observations on account of the interest attaching to them; *not* as evidence to show that the majority of shooting stars never pass out of the earth's atmosphere. Such evidence is not required—the fact being mathematically demonstrable.

Intellectual Observer for October 1867.

II.

Although there was no display of the November meteors last year [1867] in any part of Europe, yet the calculations of astronomers respecting the hour and character of the star-fall accorded very closely with the results actually observed. In the West Indies and in North America the display was well seen, and from the hour at which the maximum occurred, it is readily calculated that had the morning of November 14, 1867, been clear in England, we should have seen the commencement of the display, but not its more brilliant part. The maximum would not have been visible in our latitudes further east than the middle of the Atlantic. The mean of the hours which observers in America and the West Indies assigned to the occurrence of maximum display, differed less than two hours from the calculated epoch, a correspondence which must be looked upon as highly satisfactory, when it is remembered that our new views respecting the nature of the meteor-zone are such as largely to enhance the difficulty of predicting the hour at which the display will occur.

Further, it is clear from the evidence which reached us from America that the part of the zone of meteors through which the earth passed last November was very little inferior in the density of meteoric aggregation to the portion passed through in November 1866.

The most satisfactory news which reached us respecting the shower of 1867 was included in three letters, one from Commander W. Chimmo, who observed the shower off Martinique, another from Captain Stuart, who observed the shower at Nassau, and the third from Professor Daniel Kirkwood, who observed the shower at Bloomington, Indiana.

Commander Chimmo, while sitting on the bridge of H.M.S. 'Gannet,' saw an immense number of bright sparks falling into the sea, apparently close to the ship. 'I thought they came from the ship's funnel,' he writes, 'because they resembled the sparks caused by the burning of wood.' But having seen a brilliant meteor bursting in the east, he called the attention of the first lieutenant and master to the phenomenon. These officers were on the bridge at the time, and they saw that the meteoric shower was falling rapidly and perpendicularly, a brilliant meteor every now and then bursting and illuminating the whole heavens. The spot of cloud from which the meteors fell was only about one-sixteenth part of the whole heavens, a heavy nimbus cloud covering the rest of the sky. Commander Chimmo was unable to make distinct observations because the ship was just entering a strange harbour, and he and his officers were obliged to withdraw their attention from the progress of the display. He states that at Trinidad the shower was much better seen, no less than 2,000 meteors having been observed between two A.M. and daylight. The meteors were numerous in the N.E., as seen from Trinidad, and described arcs of 60 degrees. Some were reddish, others green, and one of a bright fiery purple.

Now, there are several points in Commander Chimmo's observation which are well worthy of comment. First, he saw the meteors falling perpendicularly. This is very different from what happened when we were watching the display in England on November 14, 1866. But when the aspect of the sidereal vault is calculated for Martinique at the hour of Commander Chimmo's observation (about half-past five in the morning), it appears that the radiant-point was very close to the zenith, so that all the meteors would seem to be falling perpendicularly towards the horizon. Again, from the splendour of the display seen by Commander Chimmo, it may be concluded that he watched the shower nearly at the epoch of maximum intensity. This is confirmed also by what he states respecting the star-shower seen at Trinidad, for although 1,600 meteors were seen there between two A.M. and daylight, only 693 were counted before half-past five A.M., and very soon after six o'clock the approach of daylight must have put an end to the display; so that within less than an hour upwards of 900 meteors must have been seen.

Captain Stuart, at Nassau, observed the display under more favourable circumstances. For Nassau lies 16° 11′ further west than Martinique, so that the hour of maximum display occurred one hour earlier as respects local time. From Captain Stuart's statistics we may judge that he saw the star-shower from its true commencement to its true end. In other words, the commencement of the shower, as seen by him, was not due to the circumstance that Nassau was coming round from the sheltered to the exposed half of the earth's globe, but to the fact that the earth had begun its passage through the meteor-zone; and, in like manner, the termination of the shower, as seen by Captain Stuart, was not caused by the coming on of daylight, but by the fact that the earth had passed through the meteor-zone. This is an important circumstance. In England, in 1866, we did not see the true commencement of the display, though the weather was clear throughout the night of November 13–14. When the earth really began to pass through the meteor-band on that night I have not been able satisfactorily to determine. The news we had from Kishnagur showed that the display had begun at ten P.M. (Greenwich time) on the night of November 13, at which hour England was on the sheltered half of the earth. But the observer who sent us the account of the display, as seen from Kishnagur, did not see the commencement of the shower, having only begun observing when the shower was already in progress.

It follows from the mere fact that Captain Stuart saw both the true beginning and the true end of the display that the part of the zone traversed by the earth in November 1867 was considerably thinner than the part traversed in 1866. The table on the next page exhibits the most important of his results. It must be noticed that the observer, an intelligent nautical man, 'was not favourably placed for an extensive view of the heavens,' and two other observers counted no less than 1,100 meteors between 2h. 30m. and 4h. 45m., up to which hour Captain Stuart had counted only 800.

There is a mistake somewhere in the published table, the first and third columns not corresponding exactly together. However the mistake, wherever it may be, is unimportant, only affecting a five-minute period, and I believe that the following modification of the table represents what the published table was intended to express:—

OBSERVATIONS OF THE METEORIC SHOWER OF NOVEMBER 14, 1867, taken at Nassau, N. P. Bahamas (lat. 25° 5′ N., long. 77° 22′ W.), by Captain Stuart, Deputy Inspector of Lighthouses.

Local Time	Proportion of Sky Clear	Length of each Period	Number of Meteors seen in each Period	Total from Commencement
A.M.				
Before 3 0	.5	—	—	45
3 15	—	15	52	97
3 30	—	15	32	129
3 40	—	10	34	163
3 50	.6	10	49	212
4 0	—	10	118	330
4 10	—	10	125	455
4 15	—	5	102	557
4 20	—	5	73	630
4 25	—	5	69	699
4 30	—	5	66	765
4 35	—	5	21	786
4 40	—	10	52	838
4 50	—	10	64	902
5 0	.8	10	65	967
5 10	—	10	22	989

It will be seen that the epoch of maximum display occurred somewhere between 4h. 15m. and 4h. 20m. A.M. Nassau time, say at 4h. 18m. This corresponds to about half-past nine Greenwich time, and it will be remembered that astronomers in England assigned half-past seven as the hour of maximum display.

Captain Stuart's account is confirmed by the statements of Professor Kirkwood. Assisted by Professor Wylie and several students, he kept watch for meteors from 9h. 15m. P.M. to 5h. 15m. A.M., at the Indiana University, Bloomington. The night was very unfavourable for observation, the sky being obscured by so dense a haze that scarcely any fixed stars, except those of the first magnitude, were visible. It is remarkable that under such circumstances any shooting-stars should have been seen at all, and we may fairly conclude that, had the night been favourable, a display equalling, if not excelling, that which we saw on November 13–14, 1866, would have been observed by the Professor and his fellow-watchers. The results actually observed were as follow:—

November 13, from	9h. 15m. to 12h. 0m.,	1 meteor.
„ 14, „	0h. 0m. to 3h. 15m.,	75 meteors.
	3h. 15m. to 4h. 15m.,	351 meteors.
	4h. 15m. to 5h. 15m.,	98 meteors.

The time is Cincinnati time, differing from Greenwich time by about 5h. 38m. 'It will be noticed,' says Professor Kirkwood, 'that 351, or two-thirds of the whole number seen in eight hours, were observed between 3h. 15m. and 4h. 15m. The maximum occurred about 3h. 45m., when the rate was twelve per minute. All the meteors, with one or two exceptions, were conformable' (that is, belonged to the November shooting-star system). 'Two or three were sometimes seen simultaneously, and a tendency to appear in clusters was distinctly noticed. A very remarkable meteor was observed in Leo, a little above the Sickle, at about 3h. 40m. It was stationary, and continued visible between two or three seconds. It was at first small, but increased rapidly in magnitude, until, just before extinction, it surpassed Regulus, the only star in the Sickle then visible through the haze. This meteor was undoubtedly near the radiant.'

The epoch of maximum display assigned by Professor Kirkwood corresponds to 9h. 23m. Greenwich time, and, therefore, agrees very closely with the result we have already deduced from Captain Stuart's observations. This agreement is the more remarkable, because Nassau is upwards of 1,200 miles from Bloomington, so that the meteors seen in the two places belonged to different parts of the meteor-band. The agreement in the position of the region of densest meteoric aggregation, indicates a stratification of the meteor-system, and corresponds, therefore, with the views respecting its structure which I put forward in the preceding paper.

But now we have to consider some very remarkable conclusions which flow from a comparison of the observations made upon the November meteors in the years 1866 and 1867. So long as it was supposed that the band of meteors held a position in space nearly identical (save as respects the inclination of the meteoric orbits) with that of the earth's orbit, it was easy to explain the occurrence of showers in two, or even in three, successive years, without assuming a very remarkable extension of the meteoric cluster to which the more striking displays were supposed to be due. This cluster was supposed to circle around the sun in a period either slightly exceeding or slightly falling short of a year, so that after one nearly central passage, the next encounter between the earth and the meteor-system would take place not very far on either side of the region of densest aggregation. We say not very far, and it will be seen presently that the expression is justified when the real dimensions of the meteor cluster come to be compared with those we are considering. But, in fact, the distance between the points at which the earth was supposed to cross the meteor-system in successive years was very little less than the thirty-third part of the circumference of the earth's orbit, so that the space we have spoken of as *small*, really amounted to about fifteen millions of miles.

But now let us consider the true figure of the meteor-orbit. We suppose that most of our readers are familiar with the evidence which has led astronomers to recognise the fact that the meteors travel in a period of 33¼ years. We have not space even to summarise the process of inquiry pursued by Adams—for to Adams alone is due the discovery in question—but we may remark that the result he obtained is not a dubious one. Those who understand the nature of the problem he dealt with, and the exact manner in which mathematical analysis enables us to deal with such a problem, know that the agreement between the nodal shifting of the meteor-band with that due to an orbit having a 33¼ years period, is sufficient to prove beyond a doubt that this period is the true period of the system.

So soon as the period is known, it becomes at once possible to estimate from the assigned position of the radiant-point the true direction in which the meteors cross the earth's orbit, and thence the exact position of the orbit. In this part of the work, a part which is within the powers of very inferior mathematicians, Professor Adams was anticipated, we believe, by Leverrier, his co-labourer of old, and by others. A slight difference exists between the results obtained by different mathematicians—a difference wholly due to the different radiant-points they adopted. Taking Adams's results, founded on the supposition that the radiant-point was situated in R.A. 149° 12′, and N. Dec. 23° 1′, we have the following elements for the meteor-system:—

Period	33·25 years (assumed).
Mean distance	10·3402
Eccentricity	0·9047
Perihelion distance	0·9855
Inclination	16° 46′
Longitude of node	51° 28′
Distance of perihelion from node .	6° 51′

Motion retrograde.

Schiaparelli's results differ very little from these, save as respects the two following elements:—

Inclination	17° 44′
Distance of perihelion from node	4° 57′

It is important to notice that a very trifling difference in the assumptions made with respect to the position of the radiant-point, affects these two elements appreciably.

Fig. 6 represents the orbit of the November meteors, according to the estimate of Professor Adams. E E′ is the earth's orbit, crossed by the meteor-system, at the point marked ☋, which indicates the descending node of the meteoric orbit upon the ecliptic. The perihelion of the

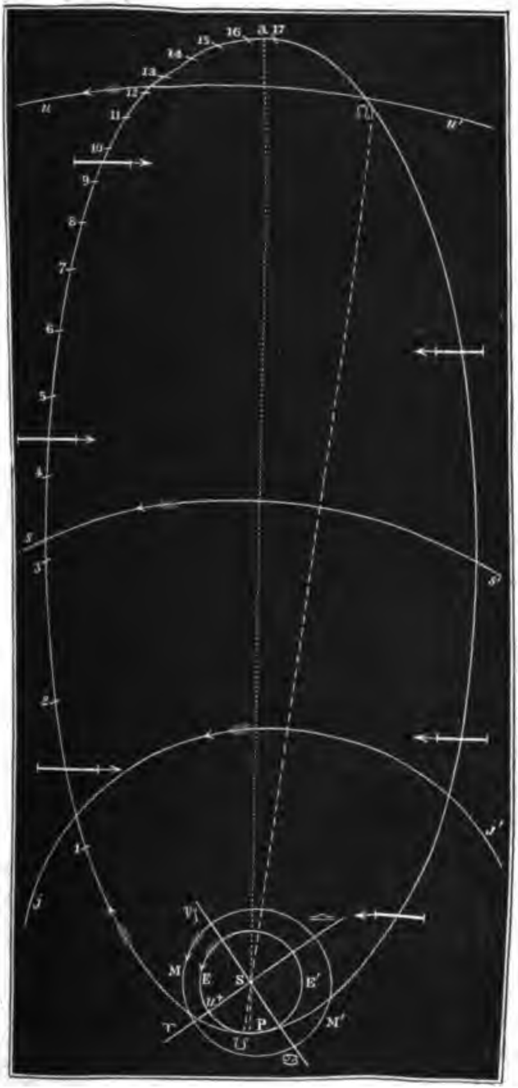

Fig. 6.—The orbit of the November meteors.

meteor-system is at P, the aphelion at *a*. The orbits of Mars, Jupiter, Saturn, and Uranus are indicated by the letters M M′, *j j′*, *s s′*, and *u u′* respectively. The arrows indicate the direction of the orbital motions. It must be observed that the portions of the orbits of Jupiter, Saturn, and Uranus are laid down with their proper eccentricity. For instance, the centre of the arc *u u′* is at u^{+}, not at S. The eccentricity of the orbit of Mars will be obvious at once. The line ☋ ☊ is that in which the plane of the meteoric-zone intersects the plane of the ecliptic. It will be noticed that the ascending node lies close to the orbit of Uranus. This approach to coincidence came out exactly as represented, by the mere process of careful construction. As there is every reason to believe that the introduction of the meteors to their present position was due to their having approached Uranus very closely (the epoch assigned by astronomers to the appulse is A.D. 126), it follows, of course, that their orbit ought to indicate an agreement of this sort—for, having once assumed an orbit through the attraction of Uranus, they were compelled from that time forth always to pass, once in each revolution, through the point at which the encounter took place.

The part of the meteor-orbit on the right of the line ☋ ☊, in the figure, is supposed to lie above the plane of the paper, the remaining part below that plane. The lines tipped with arrow-heads indicate the amount by which the orbit is depressed below or raised above the ecliptic-plane on the scale of our figure. In this indication only the part of each line between the two cross-lines is to be considered. It will be noticed that when these effects of the inclination of the meteoric orbit are attended to, the orbits of Jupiter and Saturn are found to pass at a considerable distance from the meteoric orbit; in fact, neither Jupiter nor Saturn can ever approach the meteors at a less distance than about eighty-five millions of miles, even at those points where, in the figure, the orbits of these planets appear to intersect the meteoric orbit. Mars, in like manner, cannot approach the meteors by a less distance than twelve or thirteen millions of miles.

The division-marks round the orbit of the meteors indicate the arcs over which they pass in successive years, starting from *p*. Thus, in one year a meteor will have reached the point 1, in two the point 2, and so on; and it will pass the aphelion-point, *a*, after sixteen years and five-eighths have elapsed from the epoch of perihelion-passage. It will be observed that the rate of motion in aphelion is very much less than the rate in perihelion; in fact, in one year after perihelion passage a meteor traverses upwards of five hundred millions of miles, whereas, in one year near the aphelion-passage a meteor travels over about forty-five millions of miles only.*

I believe that the figure which accompanies this article is the first in which the true relation of the meteoric orbit to the orbits of the planets (properly eccentric) has been exhibited with any approach to exactness.

If Schiaparelli's elements be adopted, the line *p a* would have to be shifted around S, through an angle of about two degrees, the end *a* moving towards the right.

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* In connection with the points marked 1, 2, 3, 4, etc., I may venture to relate a somewhat amusing anecdote. A very eminent astronomer was desirous of drawing a figure corresponding to that presented in my illustrative plate. But he was so accustomed to abstruse mathematical investigations that the simple process of construction which a far inferior mathematician would make use of—those, for example, which I used in constructing the figure of the orbit—did not occur to him. Instead, therefore, of laying down the ellipse from its known axis major and eccentricity, he adopted a very novel and somewhat laborious process, corresponding to the process of breaking a butterfly on the wheel. He actually *calculated* every one of the points marked 1, 2, 3, etc., deducing the radius-vector and eccentric anomaly, in each case, by a complicated process of approximative calculation; and then, when he had marked in all these points, he took his ellipse through them. It is as though a sum in addition were worked by the differential calculus.

One of the most interesting facts revealed during recent years, is the circumstance that the orbit of the November meteors exhibits a very close accordance with that of a telescopic comet discovered by Tempel, early in 1866. The extent of the agreement will be perceived by comparing the following elements of the comet with those assigned above to the meteoric orbit:—

Period	33·18 years.
Mean distance	10·3248
Eccentricity	0·9054
Perihelion distance	0·9765
Inclination	17° 18′
Longitude of node	51° 26′
Distance of perihelion from node	9° 2′
Direction of motion	Retrograde.

These are the elements assigned by Dr. Oppolzer. It will be noticed that the inclination lies between the values assigned by Adams and Schiaparelli to this element. The distance of the perihelion from the node differs 2° 11′ from Adams's estimate, and 4° 5′ from Schiaparelli's. A very slight difference in the assumed position of the radiant points of the November meteors would have brought these elements into perfect agreement. The period of 33·25 years assigned to the November meteors accounts for a large proportion of the remaining discrepancies, which, however, are exceedingly minute as it is; in fact, the *figure* of the orbit assigned to the comet would correspond so closely with that assigned to the November meteors in the plate, that it would not be possible to distinguish one from the other on the scale of that figure. The difference of *position* would correspond to a shifting of the line *p a* around S, the end *a* moving towards the left, through an angle of 2° 11′. The latter difference is one which could be wholly accounted for, not only by assuming a very minute error in the determination of the radiant-point of the November shooting-stars, but by assuming very slight errors in the observations made upon comet I., 1866, during the time that it continued visible in our skies. No reasonable doubt can exist that the meteors and the comet form a single system. And, by the way, it is worth noticing that, as the comet passed its perihelion early in January 1866, it had travelled the best part of the way towards the point 1 in November 1866; so that we passed through a point (☋) removed some four hundred millions of miles from the nucleus of the comet. In 1865, on the contrary, the earth passed through a point much nearer to the head of the comet, but in advance of it.

And now let us consider for a moment the actual volume of the space which is occupied by cosmical bodies, aggregated with greater or less density, and forming what we now know as the November meteor-system. In the first place, we must, I think, dismiss the notion that there are gaps or breaks in the system. A consideration of the well-authenticated observations which have been made upon the meteors suffices to show that, although there are variations in the density of meteoric aggregation, and also in the thickness of the ring of meteors in different parts of its circumference, yet these variations take place in a continuous manner; in other words, there are neither sudden increments nor sudden decrements in the density of meteoric aggregation.

We have, then, a ring of meteors, forming an ellipse of the figure presented in our illustrative plate. The major axis of this ellipse is about eighteen hundred and eighty-five millions of miles long; its circumference little less than forty-four hundred millions of miles long. The ring is probably flattish, but is certainly variable in thickness. What its width may be we cannot tell. Our supposition that the ring is flat involves, of course, the conclusion that the width of the ring is greater than the thickness, and we think there can be very little doubt on this point. The disturbing forces to which the ring is subjected are such as must tend far more to an increase of width than to an increase of thickness. In the preceding paper I pointed out that the consideration of the phenomena presented during the display of November 13–14, 1866, sufficed to show that the portion of the meteor-system through which the earth then passed was certainly not less than 100,000 miles thick, and 6,000 miles broad. We may fairly assume that the breadth of that part of the ring is some million or so of miles. Now, we have seen that the part of the ring traversed in 1867, although quite as densely crowded with meteoric bodies, was not so thick as the part traversed in 1866. Applying the same method to the determination of the thickness that I used in the latter case, we obtain a thickness of about 60,000 miles. We may fairly assume a breadth ten times as great. Further, the part of the ring we passed through in 1866 had moved off upwards of 530,000,000 miles in November, 1866. The whole of this long arc was occupied by a portion of the ring, which we may suppose to have thinned off gradually from a thickness of 100,000 miles, at one extremity, to a thickness of 60,000 at the other. Assigning to it a mean thickness of 80,000 miles, and a mean width ten times as great, we obtain for the volume of the portion of space thus shown to be occupied by meteors, the following imposing dimensions :—

$$80{,}000 \times 800{,}000 \times 530{,}000{,}000 \text{ cubic miles};$$

that is, no less than thirty-four millions nine hundred and twenty thousand millions of millions of cubic miles !

We have spoken of densely aggregated meteors in dealing with that portion of the system which supplies brilliant star-falls. But this term must be understood relatively, not positively. Even the appearance of ten or twelve meteors in a second, which would correspond to a very brilliant shower, would not indicate a very close aggregation of the members of the meteoric system; for, in a second, the earth passes over eighteen miles, and the meteors traverse about twenty-five miles in the same period.[*] Hence, making due allowance for the inclination of the meteoric orbit, we find that an interval of one second corresponds to the passage of the earth through about forty miles of the meteor-system; and twelve minute bodies along a line of forty miles could hardly be said to be very closely aggregated. But this would correspond to the case of all the twelve meteors appearing in exactly the same part of the heavens. As, in fact, they appear in different parts, we must further take into consideration the circumstance, that meteors may be visible simultaneously at places removed some 1,500 miles from each other; in fact, the consideration of Figs. 4 and 5 illustrating the preceding paper will show that our twelve meteors must be supposed to be contained within an elliptical tubular space, the length of the tube being forty miles, the major axis of the ellipse 1,500 miles, and the minor axis varying according to the hour of display. As the weight of twelve November meteors would in general hardly exceed a few pounds, we can see that the mean density of the meteoric ring is indefinitely small even in the richest parts of the system.

Space will not permit me to dwell, as I should wish to do, upon the startling considerations suggested by the examination of the November meteor-system. Wonderful as is the scale of the system itself, it is rather what the system suggests respecting the interplanetary spaces which most strikingly attracts our attention. Look at the orbit of the ring,

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[*] The following simple formula is convenient for determining the relation between the velocities (v and v') of two bodies at the same distance r from the sun, and travelling in orbits having mean distances a and a' respectively.

$$v^2 : v'^2 :: 2aa' - a'r : 2aa' - ar.$$

as pictured in our illustrative figure, and consider how minute the *à priori* probability that the earth should encounter such a ring of meteors, if there were but one. The chances may be reckoned at millions on millions to one against encounter. And therefore the chances are millions on millions to one that there is more than one such ring, and the balance of probability is in favour of there being millions of such rings. We know that this ring presents no sign of its existence (save at the epoch of encounter), even in the most powerful telescope. We know, also, that the comet which is associated with it has escaped detection for hundreds of years, and might very well have escaped detection for many more hundreds. Therefore we may safely assert that, in the mere non-detection of any signs of the existence of other meteor-rings, there is absolutely no argument whatever against the theory (in itself a highly reasonable one) that there are millions of rings similar to the November meteor-system. But I must refrain from pushing further the speculations suggested by theories of this sort.

Many of my readers will, doubtless, be anxious to know what prospect there is of a display of meteors being visible on the 14th of November 1868. I fear there is but little. Calculating from the display of 1866, we should assign half-past one or a quarter to two (in the afternoon) as about the hour at which the earth will pass through the richest stratum of the ring-system. Calculating from the display of 1867, we should assign half-past three or a quarter to four as the hour of passage. At either epoch England will be upon the sheltered hemisphere of the earth.

In fact, it is not likely that the display will be well seen by practised observers anywhere. In New Zealand it may be seen, though the position of New Zealand on the earth's southern hemisphere is unfavourable (for reasons suggested in the preceding paper). It is possible that a few travellers who may happen to see the phenomenon from various parts of the Pacific in which (if the views above expressed be correct) the display will be visible, may think it worth their while to report their observations. On the whole, however, it is more probable that we shall hear nothing of the November shooting-stars of the year 1868.

It is just possible that the form of the ring-system may not be so regular as we have been supposing. In this case the hour of display would not correspond to the above calculation, and we might even see the shower in England. The chance of its occurring on the morning of the 14th is about equal to that of its occurring on the morning of the 15th of the present month, and observers should therefore watch for meteors on both nights.

The Student for November 1868.

GUAGING THE NOVEMBER METEOR-STREAM.

Last year [1868] I discussed the figure of the November meteor-orbit, showing how we learn from the researches of Professor Adams that the meteoric-system extends far out in space beyond the orbit of Uranus. This year I wish to deal with another and equally interesting feature of the November meteor-system, namely, the varying depth of the stream of cosmical bodies of which it consists. We have been fortunate enough to obtain accounts of the display during three years in succession, and these accounts are of such a nature that we can determine the hours at which the display has commenced and terminated *for the whole earth*, as distinguished from the apparent commencement or termination at particular places. And the information thus secured serves to add considerably, not only to the interest with which we regard the whole subject of the November meteors, but to that with which we look forward to the display of the present year [1869]. It is important that observers should be aware of the fact that this year's display, if it should be well observed, will serve to confirm or to disprove certain remarkable conclusions which have been drawn from the observations made last year.

Let us briefly consider what we have hitherto learned respecting the depth of the meteoric system at those particular parts of its length at which the earth has traversed it during the last few years; and, then, by combining togegether the information thus obtained, let us endeavour to form a conception of the *shape* of the meteor-stream.

In November 1867 (see 'Intellectual Observer' for that month), I examined, at considerable length, the evidence we had obtained respecting the part of the system traversed by the earth in 1866. Since that paper was written I have obtained evidence on a point then referred to as doubtful. I have learned that in India the display began some hours before four A.M., local time. Therefore the thickness of about 100,000 miles, which I then assigned to the meteor-system at that part of its course, may be increased to some 110,000 or 115,000 miles.

In November 1868, I dealt with the earth's passage of the meteor-system in 1867. The evidence from America served to prove that a very fine display had been observed; but that the display did not last so long as in 1866. And the conclusion to which we were led was that the thickness of the meteor-stream, where the earth then traversed it, was little more than 60,000 miles.

Thus we seemed to have evidence of a thinning off of the meteor-system. And remembering that the comet, with which the system seems in some inexplicable way to be associated, had crossed the earth's orbit shortly before the display of November 1866, we might unreasonably have been led to the conclusion that the thickness of the meteor-system diminished in proportion to increased distance from the cometic nucleus. This inference would have led us to expect in 1868 a display of a very unimportant character, and visible over a very limited area. The particular region over which the display was to be looked for was not a promising one. 'In fact,' I wrote at the time (and, in a letter sent soon after to 'The Times,' Mr. Hind, the superintendent of the 'Nautical Almanac,' expressed a very similar view), 'it is not likely that the display will be well seen by practised observers anywhere. In New Zealand it may be seen, though the position of New Zealand on the earth's southern hemisphere is unfavourable. It is possible that a few travellers, who may happen to see the phenomenon from various parts of the Pacific in which the display may be visible, will think it worth their while to report their observations. On the whole, however, it is more probable that we shall hear nothing of the November shooting-stars of the year 1868.'

Therefore, when news was received from various parts of England that the display had been well seen, the explanation to which astronomers somewhat hastily jumped was on this wise:—The November meteors, traversing their wide orbit around the sun, are liable to be attracted from their normal paths by the influence of Jupiter, Saturn, and Uranus—all three being giant members of the planetary system—and as all the meteors travelling in a given part of the system must be subject to the same influence, it is clear that the meteor-stream will be liable to changes of figure, resembling the vibrations which pass round an elastic hoop that has been sharply struck. And though these vibrations, considered with reference to the whole orbit of the meteors, might appear as insignificant as the scarcely perceptible vibrations of our illustrative hoop, yet they must shift the meteor-stream through spaces of enormous real extent. So that if the earth reached in November a part of the system where the range of vibration from the true orbit was a maximum, the epoch of the display might be hastened or delayed by several hours. And thus the unexpected occurrence of a display in November 1868 might fairly be accounted for.

Although this reasoning is undoubtedly plausible—nay more, though it is undoubtedly true that the meteor-system must be subject to vibrations of the kind considered, yet it very soon appeared that the occurrence of a display in November 1868 was due to a cause of quite a different character.

Remembering the evidence obtained in 1867, of a thinning off in the meteor-stream, it will be evident that, supposing the part passed through in 1868 to be correspondingly diminished in thickness, the display could not have lasted more than two or three hours; and, therefore, being, as I have said, well seen in England, it would necessarily have been invisible (occurring in the daytime) in America.

But news was received that the display had been well seen in the United States.

It was at once evident, therefore, that the process of thinning off had been followed by a contrary process, and that in fact the thickness of the stream where the earth crossed it in 1868 was not only greater than at the part traversed in 1867, but even than at the part traversed during the great display of 1866.

This result is so interesting, and serves so largely to enhance the interest with which we look forward to the display of the present year [1869], that it may be well to consider somewhat closely the evidence on which it rests.

So far as the display in England is concerned, we have very satisfactory evidence. Let us take Professor Grant's description of the shower as observed at Glasgow.

Until about half-past two on the morning of November 14th, the sky was somewhat overcast, but it was evident even then that a shower was in progress, as a meteor would be seen every now and then to flash across an opening between the clouds. At half-past four it was clear in all directions, and it became easy for the observers to convince themselves that the meteors which appeared in every part of the heavens belonged to the November system. In every instance the course of the meteors was found to emanate from the radiant of the November system. The meteors were commonly white, but in some instances a trace of red could be recognised. It was noteworthy, however, that no trace was visible 'of the beautiful green which formed so interesting a feature of many of the meteors of November 1866.' This peculiarity is well worth dwelling on for a moment. It would be an interesting circumstance if we could trace a systematic law of change in the character of the meteors, according to their distance from the cometic nucleus of the meteor-system.

Many of the meteors were large, 'three or four exceeding Jupiter in brightness, but not equalling the planet Venus, which was shining with intense brillancy in the east, and formed an excellent standard of comparison for estimating the brightness of the larger meteors.' Size again—that is (1) the average size of the meteors, and (2) the size of the largest which make their appearance—is a feature which should be carefully attended to in observing the coming display. We want all the evidence we can get to guide us towards a solution of the difficult questions suggested by the meteors; and we must not be deterred, by considerations of the apparent insignificance of this or that feature, from recording every phenomenon which may by any possibility afford a useful hint.

But our chief concern at present is with the thickness and density of the meteor-stream.

Professor Grant and his assistant noticed that the shower sensibly increased, from 4h. 30m. to 4h. 56m., and 'as it appeared very desirable to endeavour to ascertain the time of its maximum,' he 'proceeded, in conjunction with Mr. John McKinnel, the junior assistant, to count the number of meteors which might become subsequently visible.' It would appear from the resulting numbers that the shower attained its maximum at about a quarter past five. But there was no such sharp accession of richness as had been observed in 1866 or 1867. The following table, which indicates the number of meteors seen in successive intervals of five minutes, commencing at four minutes before seven, serves to prove this:—

	h.	m.		h.	m.	Meteors		h.	m.		h.	m.	Meteors
From	4	56	to	5	1	22	From	5	31	to	5	36	16
„	5	1		5	6	28	„	5	36		5	41	12
„	5	6		5	11	27	„	5	41		5	46	14
„	5	11		5	16	27	„	5	46		5	51	11
„	5	16		5	21	16	„	5	51		5	56	13
„	5	21		5	26	20	„	5	56		6	1	8
„	5	26		5	31	21	„	6	1		6	6	19

We shall presently see that Professor Grant observed neither the real beginning nor the real end of the display.

We turn next to the observations made in the United States. Professor Kirkwood records them, but was unfortunately unable to take part in them as on former occasions. *On the morning of the* 13th, Professor Wylie observed 165 meteors, of which the greater number belonged to the November system. *After sunrise*, Professor Kirkwood bethought him of an observation made by Humboldt in 1799, and 'standing in the shade, on the western side of a building, watched the vicinity of the radiant, hoping to see some of the largest of the meteors.' He saw five or six, and Mr. Maxwell, a tutor in the State University, who watched with him afterwards, 'saw *one*, beyond doubt, and three others less certainly.' This fact is interesting, as confirming Humboldt's assertion that the meteors can be seen in the daytime.

On the night of the 13th the display was well seen. A committee of the senior class in the University kept watch from 11 o'clock P.M. till 4h. 15m. A.M. (Cincinnati time), during which they counted no less than two thousand five hundred meteors. The maximum was at about half-past three, 'nine hundred meteors having been counted during the forty-five minutes immediately preceding.' This is at the rate of one hundred in five minutes, and enormously exceeds the numbers counted in corresponding intervals by Professor Grant and his assistants. Many of the meteors were very brilliant, and left long trains which continued visible for several minutes. Three or four were observed to explode, or at least to separate into several parts,—a phenomenon which had not before, so far as I know, been observed, in connection with the November meteors.

At five minutes to five the watch was renewed by Professor Wylie, who continued to observe the meteors until 6h. 11m., counting seven hundred and eighty in one hour and sixteen minutes.

There are several remarkable points to be noticed in this narrative:—

In the first place, the display began on the night of the 12th, and was still in progress at daybreak on the 14th, or more than thirty hours later. In 1866 the display did not last more than five or six hours, and in 1867 its duration was even less.

Again, the epoch of maximum display observed in the United States does not by any means correspond with the hour named by Professor Grant. The difference of time between Cincinnati and Greenwich is about 5h. 38m., so that the hour of Professor Grant's maximum (5h. 15m., on the morning of November 14th) corresponds to twenty-three minutes before midnight, November 13–14, at Cincinnati. Hence, nearly four hours after Professor Grant's maximum, the earth passed through another and a much denser part of the meteor-system.

Again, a fact was noticed in America which serves to confirm the evidence afforded by the circumstance just noticed, of a stratification of the meteoric system in that region which the earth traversed in 1868. At frequent intervals throughout the night, says Professor Kirkwood, 'a lull occurred in the display; while at other times, for a few seconds, the meteors were so numerous that they could scarcely be counted.'

But the meteors were to be seen at yet a third station, far removed both from England and from the United States; and it will be well, before summing up the evidence which last year supplied respecting the constitution of the meteor-system, to examine the facts observed at this third station, the Cape Town Observatory.

Mr. Maclear noticed the first meteor from the radiant in Leo, at 1h. 18m. Cape time. Such an observation, if made in England, would signify that the true commencement of the display had then taken place, and so would be discordant with the evidence from America. But a reference to Figs. 4 and 5, pp. 110 and 111, will at once show that the Cape only began at about that hour to be within the range of the hail of meteoric projectiles. To represent the matter in another light, the radiant in Leo rises several hours later at Cape Town than in our latitudes.

The display was not very remarkable at first, nor indeed did it at any time attain such proportions as in the United States. Still at about a quarter to three, a shower of some importance seems to have been in progress, a dense haze concealing many of the smaller ones from view. The exact time which Mr. Maclear assigns as the epoch of maximum display is 2h. 42m. Cape time. This corresponds to about 1h. 31m. Greenwich time. Here then is another maximum, occurring before Professor Grant's—in fact before the sky had cleared at Glasgow.

But it is quite clear from Mr. Maclear's account that the true maximum did not occur at the hour he names. There were, in fact, several maxima. Certainly in the minute between 2h. 42m. and 2h. 43m. more meteors were seen than in any other single minute. But if we take the interval of ten minutes beginning at 2h. 37m., we find that only thirteen meteors made their appearance; whereas in the interval of ten minutes beginning at 3h. 37m. eighteen meteors were seen.

The fact is, Mr. Maclear's observations confirm those already recorded with respect to the evidence they afford of a very decided stratification in the meteor-stream. At least that is the view which seems forced on us when we interpret what was observed in 1868, by means of what took place in the two former years. For in 1866 and 1867 there was so close an accordance between the epochs of maximum display observed in places very far apart, as to prove that the denser regions of the system were of considerable width—or, in other words, that there were real strata of meteoric aggregation. In 1868 we had no evidence of this sort, though we have none disproving the notion that the part of the system then traversed was also stratified. It is still possible, however, that the earth passed in 1868 through a succession of clustering aggregations rather than through strata of aggregation. It is to be hoped that the observations which may be made this year will serve to clear up this difficulty.

Let us now sum up the evidence we have respecting the portion of the system traversed in 1868; and then, comparing that evidence with what we know of the regions traversed in 1866 and 1867, let us endeavour to picture to ourselves the solid figure of the arc of the meteor-system extending from the place of Comet I, 1866, to the region traversed in 1868. We may add the observations made in 1865, though these applied to a part of the meteor-system which is travelling in front of the comet.

The earth occupied at least thirty hours in traversing the meteor-stream. As the passage was oblique we must not take the earth's orbital velocity of some sixty-five thousand miles per hour; but we must reduce our estimate to about eighteen thousand miles per hour, that being about the value of that portion of the Earth's velocity which is carrying her *directly* through the meteor-stream. This gives to the stream a depth of no less than five hundred and forty thousand miles. So that the part traversed by the Earth in 1868 was more than five times as deep as the part traversed in 1866, and nearly ten times as deep as the part traversed in 1867.

These results, combined with what is already known of the figure of the meteoric orbit, enable us to form some conception of the real figure of the meteor-system in space. It must of course be remembered that as the meteors circle round in their orbit, the condensation occupying successively different parts of the long oval pictured in Fig. 6, the configuration of the system must vary very strikingly. For example, when the condensed part of the system is near aphelion the whole of the richer part of the system around the condensation must be compressed along a much shorter arc. We may measure this richer portion (in arc-length) by estimating the time which the last straggler belonging to it would take in reaching the position occupied by the leading member of the vanguard; and this time we may assume to be very nearly constant. This being so, it will be obvious from a moment's study of the orbit, that when in aphelion the whole of the richer portion of the system may scarcely occupy one-tenth part of the space which the same portion comes to occupy when its condensation is travelling past perihelion.

It is only, therefore, at a special time that the accompanying drawing* can be taken as illustrating the configuration of the part of the meteoric-system which has during the last few years passed the descending node near the Earth's orbit:

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* Fig. 7 is not the drawing which originally illustrated the article. It has been extended so as to exhibit the parts actually traversed (since the article was written) in 1869, 1870, and 1871. See next paper and note at its close.

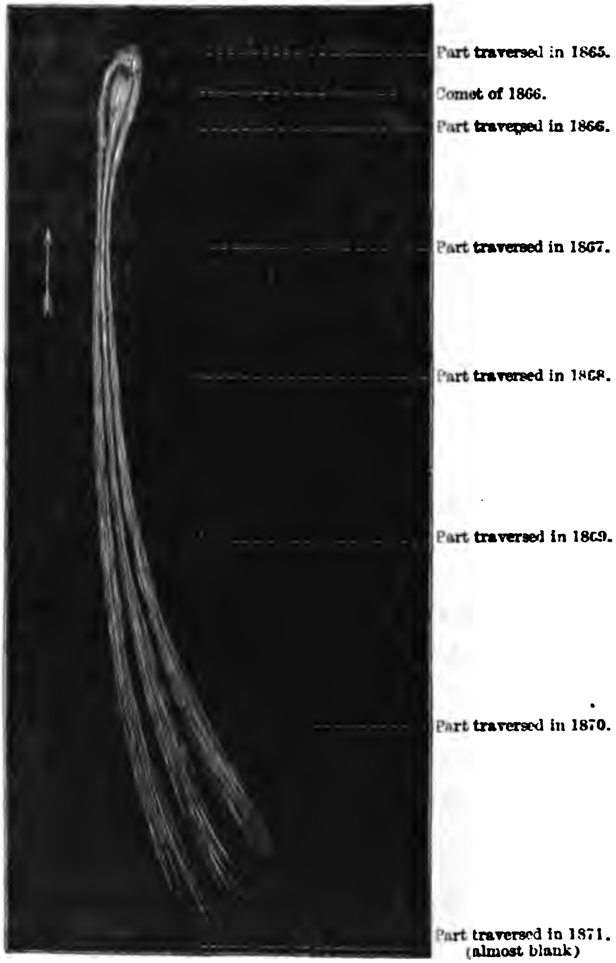

Fig. 7.—Ideal view of Tempel's comet and the November meteor-system, greatly exaggerated in cross-section.

at the present time, in fact, the meteor-system may be supposed to occupy such a position as is here depicted. It will be noticed that the cross section has been made to vary according to the guagings obtained above. But it will be understood that it is absolutely impossible to indicate in a satisfactory manner the true relations of the system; because the cross section, if laid down according to the real dimensions of the system, would be almost evanescent unless the orbit were represented on a very large scale indeed. The figure, therefore, must be accepted as rather intended to indicate that the depth of the system varies according to such and such a law, than to present a true picture of the meteoric system.

The comet (whose dimensions are enormously exaggerated) occupies the position indicated by the observations of 1866. No stress is to be laid on the connection indicated between the comet and the meteor-system; because we are altogether ignorant what the real connection may be. At present, indeed, there are few circumstances more perplexing than the observed association between comets and meteor-systems. That in several instances a meteoric ring should occupy the exact position of a cometic orbit can hardly be supposed to be merely an accidental coincidence. Therefore, some sort of association is indicated; but what the nature of the association may be, by which flights of solid bodies are connected with gaseous comets, is a riddle whose solution no information we at present possess enables us even to guess at.

One circumstance which has not hitherto, so far as I know, been considered (though it is so intimately associated with the inquiries of Schiaparelli, Hoek, and others, that I am prepared to find I have been anticipated in dealing with it) seems to bear importantly on the relation in question.

It is well known that nearly all the comets which travel in periodic orbits around the Sun, have been brought into their present subordination to the solar attraction by the influence of the giant planets which travel outside the orbit of the asteroids. Each of these planets probably has its own family of comets, though hitherto we have only been able to satisfy ourselves respecting the existence of such a family in the case of the planet Jupiter. We know that a large number of comets have their aphelia close to the orbit of Jupiter, and we recognise the meaning of this when we remember that a comet travelling from outer space along a course which would bring it near to the giant mass of Jupiter, would be forced by his attraction (under ordinary circumstances) into an orbit having its aphelion not very far from the scene of the encounter.

Now it has been shown that Comet I, 1866, and the meteor-system associated with that comet, travel close past the orbit of the planet Uranus. The ascending node of the comet's orbit, in fact, is quite close to the orbit of Uranus, so that it is probable that the comet approaches that orbit more nearly even than the known members of Jupiter's comet-family approach the orbit of their ruling planet.

We must look then on Uranus as the planet by whose attraction the comet was forced to take up its present orbit, and astronomers having traced back the history of the comet, and that of distant Uranus, have found that in the year 126 A.D., Uranus and the comet were so close that for a brief time the comet was more under the influence of the planet's attraction than under that of the Sun's. At this time it was, then, that the comet was forced to travel on its present orbit. And it was by the merest accident that this orbit passed so near as it actually does to the Earth's orbit. Now where were the meteors when that encounter took place? If they had been straggling far behind the comet like the major part of the system at the present time, they would not have been brought under the influence of Uranus as the comet was, and their paths would not afterwards have shown any approach to identity with the comet's orbit. The fact, then, that there is that singular identity in the track of the comet and of the meteors, shows conclusively that every particle of matter constituting the meteors must originally have been in the immediate neighbourhood of the comet.

It is then since the introduction of the comet into our system that the meteoric ring has been formed. Up to its encounter with Uranus, the comet and the meteoric matter had been collected within a space of very small dimensions indeed, compared with the present dimensions even of what we term the condensed part of the meteor-system. About this we may feel absolutely certain. When we inquire, however, how the dispersion came about, we find ourselves surrounded with difficulties. Passing over the physical distinctions which seem to dissociate the meteors from their cometic companion, it is by no means easy to explain, in accordance with the laws of motion, the enormous extension at present attained by the meteoric system. If we suppose such a diversity of distance between Uranus and the various parts of the meteor-system at the epoch of encounter as would result in differences of velocity sufficing to account for the present dispersion of the system, it becomes difficult, as already shown, to explain how it was that the whole of the system was forced into the same (general) orbit. If, on the other hand, we assume a very close condensation of the meteoric particles, it becomes by no means easy to understand the dispersion of the system along an orbit whose circumference is upwards of 4,000 millions of miles in length, in the comparatively short interval (1,743 years) which has elapsed since the system was first forced to follow its present course.

We seem almost driven to the conclusion that some other force than gravitation has been at work in causing the dispersal of the meteoric particles.

The Student for October 1869.

METEORS AND SHOOTING-STARS.

Amongst the many surprising discoveries which have of late years rewarded the labours of astronomers, none perhaps are more remarkable than those which relate to the phenomena —once thought so insignificant—presented by 'falling stars.' Ten years ago, though the thoughtful astronomer had become convinced that these objects really belong to the domain of astronomy, doubt still rested on that theory of their nature. Men could scarcely believe that the vast depths amidst which the planets pursue their career around the Sun are the home of countless bodies which rush with even more than planetary velocity upon wide orbits round the solar orb. It seemed incredible that each of those faintly gleaming lights, passing with silent swoop across a star-group—leaving no trace of their existence and seemingly as little important in the economy of nature as a rain-drop or a snow-flake—indicates the close of a career during which the mighty orbits of Jupiter and Saturn have been encircled, nay, often the utmost limits of the known planetary scheme overpassed by uncounted millions of miles.

Even now, when the nature of these objects has been revealed to us, and some insight afforded us into the part which they perform in the economy of nature, it seems difficult to realise the full significance of ascertained facts. The very aspect of the planetary scheme seems changed as we contemplate the results of recent labours in meteoric astronomy. Kepler and Copernicus, could they revisit this world, and, mixing as of old among astronomers, inform themselves respecting the theories now upheld, would scarcely recognise the scheme of the universe so unfolded to their view. Truly the harmony of the planetary system recognised by Kepler seems strangely marred, 'as sweet bells jangled out of tune and harsh,' by these eccentric meteor orbits. These crowds of independent orbs rushing disorderly around the Sun, in no sort resemble the 'obedient family' which Copernicus recognised in the solar system.

Many times during the last few years the history of those discoveries and researches by which meteoric astronomy has reached its present position has been recounted. It is not my purpose to describe these matters anew. But it has seemed to me that the approach of the Earth towards that great stream of meteors to which the November shower is due, will render a brief discussion of some of the most striking facts lately discovered not unacceptable even to many who look on astronomy from afar off, and regard astronomers somewhat as Indians regard their medicine-men.

We may take the November shooting-stars as typical of a class of meteor-systems, which must undoubtedly be very numerous. It is true that as the Earth sweeps on her wide orbit round the Sun she encounters few such streams as that to which the November meteors belong. As she reaches certain critical parts of that orbit she is exposed, indeed, year after year, to a species of cannonade of greater or less intensity; and occasionally the weight of metal with which she is thus assaulted is far heavier than any which she has to encounter during the second week in November. But for a systematic and continuous downpour of missiles the November stream is unsurpassed by any, except perhaps the August meteor-system. If we could count the total number of meteors which have been rained upon the Earth during the past five or six centuries, and assign each individual meteor to its proper system, I have very little doubt that the November stream would be found to have supplied a full tenth part, though the total number of systems which our Earth encounters is known to exceed one hundred.

This being the case, it may be worth inquiring whether the November system is in reality richer than the others —whether there is anything in what we know about this stream to lead us to suppose that it is more important than the rest.

It seems to me abundantly clear that the contrary is the case. We have only two means of judging of the richness and importance of a meteor-system. One is the observation of its *apparent* richness, and the individual magnitude of the meteors belonging to it. But the apparent richness alone can be but a deceptive indication of the real richness of a stream of meteors. If we were sure that the Earth plunged through the heart of each meteor-stream, we could indeed learn something in this way, precisely as we might compare the relative thicknesses of different cords by the resistance experienced in piercing them through the middle with a needle. But we have no assurance whatever that the Earth passes through the heart of a single meteor-system. It may be that if she did the results would not be altogether pleasing or satisfactory to her inhabitants, and certainly the chances are enormously against her doing so. The minuteness of the space actually passed through by the Earth on her course round the Sun—at least the minuteness of this space by comparison with the dimensions of the solar system—is not commonly appreciated. If we represent the Sun as a globe about as large as a billiard-ball, the space along which the Earth pursues her course would be represented by a thread or twine forming a circle nearly eight yards in diameter. Now, conceiving such a circle, and regarding the meteor-systems as oval hoops round the central ball, which *happen* to cross this fine circular thread, it is scarcely conceivable that in one case out of a thousand the thread would pass centrally through the substance of one of the hoops.

We can therefore infer little or nothing from the apparent richness of meteor-streams as to their real importance, because we do not know whether our Earth passes through the core of any particular stream or merely grazes its surface.

We may learn something from the average dimensions of the meteors belonging to a system, though our inferences may not be altogether reliable. So far as this point is concerned, the November meteors would seem relatively inferior to many others. They are too small to penetrate through the atmosphere, so as to reach the surface of the Earth, not one instance being on record of a November meteor affording any tangible evidence of its existence; and from the researches of Professor Alexander Herschel, it would seem that on the average the November meteors weigh but a few grains each. When we compare this with the fact that bodies belonging to other systems have been found to weigh many pounds, some even being several tons in weight, the relative insignificance of the November system in this respect will be clearly recognised.

But there is a second method by which in comparatively recent times it has become possible to guess at the importance of different meteor-systems.

The surprising discovery that many meteor-systems are associated with comets has not hitherto been fully interpreted. We know quite certainly that along the orbits of certain comets there travel myriads of tiny bodies—meteors—which we assume to be solid. But what connection there may be between the gaseous comet and its solid attendants, whether the comet gave birth to the meteors, or whether the meteors in some way or other combined along one part of the system to form the comet, has not hitherto been explained. It may be regarded indeed as one of the most mysterious facts ever discovered by astronomers that any association whatever should exist between bodies seemingly so different in their nature as comets and meteors. But *there* the relation is, let us make of it what we will. No doubt rests on the reality of the discovery; no one who understands the nature of the evidence can believe for a moment that the relationship is merely apparent, and the coincidence of orbits merely accidental. So that, in fact, it has come to be gravely questioned whether any meteor-system exists without a cometic nucleus, and whether any comet exists without a meteoric train.

Be this, however, as it may, we are at least justified in comparing together such meteor-systems as are known to be associated with comets, and inferring the probable importance of such meteor-systems from the observed brilliancy of their comet-chief.

Now, judging in this way, we should be led to conclude that the November stream, notwithstanding the wonderful magnificence of the star-showers observed when the Earth passes through the system, is in reality one of the least important of the meteor-systems. The comet with which it has been (beyond all question) associated, is so faint and small that it has never yet been discerned by the unaided eye. In a powerful telescope it appears but as a faint nebulous light, nor is it even adorned with the ordinary appendage of respectable comets—a tail. Taken apart from the significance of what we know respecting it, this comet is certainly one of the least striking objects which the telescope has ever revealed to astronomers.

On the other hand, the August meteors are associated, with a comet of distinction—with a comet which has been watched by many millions of human beings as the harbinger of some uncanny event, and has been recognised even by men of science as worthy of respectful attention. Indeed, if its approach had been anticipated and its course known, but the hour of its arrival uncertain, it is far from unlikely that men of science would have looked forward with some dread to the possible effects of its arrival. For it was one of those comets—few, indeed, among the larger sort—whose track crosses the Earth's; and had it come but a few months earlier or later, we should by this time have had the means of answering that long-vexed question whether the Earth would suffer injury were she to come into direct collision with a large comet. So that if we judged of the relative importance of the August and November meteor-systems by a reference to the relative importance of their comet companions, we should undoubtedly conclude that the August meteors are far the most important. It would follow from this that, since the November meteor-system produces showers quite as striking as any seen in August, we do not in reality see the full splendour of the August meteors, but, passing only through its edge, recognise but the scattered outliers of the system.

But this being so, those who remember the magnificent display of November meteors in 1866, will consider with amazement how grand the August system must be if it is really capable of supplying a far more splendid shower. We remember how the stars seemed to fall continuously, so that at every instant (at least during a certain interval) shooting stars could be seen in some part or other of the heavens. And we know, also, from the accounts of Humboldt and Bonpland that, sixty years before, there had been a yet grander display. If a meteor-system associated with so insignificant a comet as that of 1866 can produce these wonderful showers, how inconceivably magnificent would be the scene if the Earth passed through the heart of the August meteor-system, associated as that system is with a comet of considerable splendour!

But similar considerations may fairly be extended to all the meteor-systems which the Earth encounters. These are counted by the hundred, and though most of them seem insignificant compared with the August and November systems, yet we have seen that no opinion can hence be formed of their real importance. Some of them may as far exceed the August system in importance as that system probably exceeds the November system. Nay, we have two excellent reasons for feeling some degree of assurance in this respect; for one of these less noted systems has been associated with the comet of 1861—an object not inferior in splendour to Donati's comet—and some of the recognised systems occasionally send us visitors in the form of massive aërolites, compared with which the tiny bodies forming the August and November meteors are as small shot to the Whitworth bolts. Startling, however, as are the considerations thus suggested, it is when we pass in imagination beyond the confines of the Earth's orbit that the true significance of what we know respecting meteors and meteor-systems becomes apparent.

We have seen that our Earth really visits but a minute proportion of the solar domain. The space actually traversed by our globe as it circuits round the Sun, though enormous compared with any of our ordinary estimates of size—nay, though exceeding fiftyfold the volume of the Sun—is yet but the minutest fraction of that vast sphere over which the Sun exerts supreme sway.

Now, since the meteors are not individually discernible save when they enter the Earth's atmosphere, all our direct information respecting the condition of the interplanetary spaces is derived from the actual contact of the Earth with bodies belonging to those spaces. We obtain our information respecting the planets through their visibility, but as respects the meteors our Earth may be compared to a blind man in a shower. It is only when the meteors or meteor-systems come into actual contact with her that her inhabitants can have direct cognisance of the existence of such bodies. Let us follow out this illustration. Suppose a blind man walked a distance of ten miles, and during the whole continuance of his walk felt rain falling upon him. Would it be a reasonable conclusion on his part that the rain had fallen precisely along the track he had followed, and nowhere else? Would he not conclude, on the contrary, that the extent of country on which the shower had fallen extended probably, at least, as far from right to left as he had found it to extend in the direction of his walk? Most assuredly he would not conclude that a narrow strip, ten miles long and perhaps a yard wide, had been rained upon, but rather an area several miles wide. In other words, he would conclude that, instead of an area of a mere fraction of a mile in extent, a range of forty or fifty square miles, at least, had been visited by the shower.

It is equally reasonable to conclude that the track of the Earth is not the only part of the Sun's domain which is crossed by meteor-systems. There is no conceivable reason why that particular *hoop of space* should be visited rather than regions lying around it. And precisely as our illustrative blind man, had he stepped to the right or to the left of his actual path, would have been visited by other raindrops than those which actually fell upon him, so we may reasonably conclude that if our Earth's orbit were changed so that she travelled a few millions of miles further from or nearer to the Sun than she actually does, then she would encounter meteor-systems altogether different from those which now assail her with a shower of 'pocket planets.' To come to the point for which I have been making all along, —*the whole of the solar domain is alive with meteors.* This is the legitimate conclusion from the evidence acquired during the last few years. So long as it was thought that the meteor-systems are nearly circular, there was an escape from this startling conclusion. It was conceivable that the meteor-systems might affect the neighbourhood of the Earth's orbit, much as the asteroidal family affects the space lying between the orbits of Mars and Jupiter. But so soon as Adams and Leverrier, Schiaparelli, Tempel, and the rest, had made it abundantly evident that the meteors travel in very eccentric orbits, there remained no escape from the conclusion that the intersection of these orbits with the Earth's path is to be regarded as a merely accidental circumstance. The Earth has absolutely no power adequate to force these meteor-systems to cross her orbit. We could understand the orbit of Jupiter or Saturn being crossed by many meteor-systems, because we know that if a family of meteors were passing close by Jupiter on a course which would carry the family far away again into space, the mighty attractive force of Jupiter or Saturn would (ordinarily) suffice to force the members of that meteor family to come close to the planet before they could speed again on their course towards the Sun's neighbourhood. Whenever such an encounter as this took place, the meteor family would, for the future (and until again disturbed by the planet), travel on a path crossing or very closely approaching the planet's. But the Earth is far too small to influence in this way the motions of meteoric families. Those which approach her speed onwards with a velocity altogether beyond her control, so that, unless already travelling on a re-entering orbit passing close by the Earth's, they could never be forced by her attraction o enter on such a track.

A body coming from the stellar depths towards the Sun could no more be forced by the Earth's disturbing attraction to follow a closed curve round the Sun, than a swiftly-rushing railway train could be caused to leave the rails by the attraction of a toy magnet.

Since, then, those meteor-systems which cross the Earth's orbit are chance visitors, as it were, not drawn to their present paths by any attraction the Earth can exert, but coming of their own accord past her track, it follows that there must be for each recognised meteor-system uncounted thousands which are unknown to us because they do not approach the Earth's track. There is no escape from this conclusion. The laws of probability will not permit us to believe that, out of a moderately large number of meteor-systems in no way attracted to the Earth's orbit, a large proportion would traverse that particular track in space. To judge the number of meteor-systems as no greater than the number encountered by the Earth, would be like counting the rain-drops which fall upon a window-pane in London, and concluding that just that number and no more had fallen on the whole city.

It is this conclusion which gives so great an interest to the researches of Adams, Leverrier, and others on the November meteor-system. If we were sure that that meteor-system was the only one of its kind, or had but few fellows, we could attach no great importance to its peculiarities. They would have a certain interest, doubtless, precisely as the discovery of an asteroid has a certain interest; but they would involve no results of cosmical significance. Under the actual circumstances, what has been proved respecting the November meteors opens a field of conjecture of almost boundless extent. Whence come these uncounted millions of bodies, rushing through space with inconceivable velocity? What purpose do they fulfil in the economy of the solar system? Do any of them pour upon the Sun, as has been supposed, a hail of cosmical material, replenishing his fires and recruiting his energies? Has the mighty attractive influence of the Sun, which guides the planets on their wide circuits, this further work to perform, of gathering from out of space the material by which his own fires are fed? Or do these myriads on myriads of cosmical bodies, with all the vital forces represented by their velocity, subserve no purpose whatever in the economy of our system? Are they the chips in the great workshop of Nature, the sparks which have flown from the mighty grindstone, the shreds of clay which the giant potters Attraction and Repulsion have cast aside as useless?

This paper was accompanied by the following note:—

Our readers may be desirous of learning what are the chances that the display of November meteors will this year be worth observing. In 1866, it will be remembered, the great display lasted but a few hours. Had it occurred either a few hours sooner or a few hours later, we, in England, should not have witnessed it. In the former case we should have been on the sheltered part of the Earth—to leeward, so to speak, of the meteor storm; in the latter, though the meteors would have fallen upon portions of the atmosphere above our horizon, it would have been full daylight, and we should have seen no trace of them. In 1867 the display also lasted but a very short time, and was not visible in England. Had the shower in succeeding years lasted an equally short time, it would have been possible to tell, at least approximately, where the display would be seen this year. But since 1867 the November meteors have supplied a shower lasting many hours, though not so rich as in the former years. Last year, indeed, the shower would seem to have lasted several days, since observers noticed that on the 11th, 12th, 13th, 14th, and 15th of November, the stray shooting-stars travelled from that part of the constellation Leo which is called the radiant of the November meteors. At intervals the stars fell pretty thickly, and all the observed facts seem to indicate the justice of the view put forward by Professor Alexander Herschel (at the November meeting of the Royal Astronomical Society), that the system has separated since 1866 into three distinct strata. I had myself put forward in October a somewhat similar theory. Referring to the observations made on the meteors in 1868, I said (in the *Student* for the above date), 'There were several maxima,' 'the epoch of maximum display observed in the United States did not correspond with the hour named by Professor Grant,' of Glasgow, as the hour at which the shower reached its maximum; and further on, referring to observations made at the Observatory of Capetown,—'here then was a third maximum occurring before Professor Grant's.' In the same paper I drew an ideal picture of the system indicating the probable nature of the part to be traversed in 1869, and the great width I assigned to this part corresponded exactly with the observed event. I feel, therefore, some confidence in announcing my opinion respecting the shower this year. I believe that on the nights of November 11–15, *after twelve*, many meteors belonging to this system will be visible, and that at intervals on the nights of November 12 and 13 (that is, the nights between November 12–13, and November 13–14) there will be from midnight to dawn showers of stars, not comparable in splendour, perhaps, with the displays which took place in 1866 and 1867, nor lasting many minutes, but still well worth observing.

[This anticipation was confirmed by the event. But the display of 1870 was the last of the series inaugurated by the magnificent shower of 1866. In 1871, as already mentioned, only a few stragglers were seen.]

English Mechanic for November 4, 1870.

THE ZODIACAL LIGHT.

It cannot but be regarded as a remarkable circumstance that the nature of the Zodiacal Light should in the present state of astronomy continue to be a *quæstio vexata.* I do not here refer to the physical constitution of this object, respecting which we may possibly be unable for many years to form a satisfactory theory, but to the determination of the actual position of the Zodiacal Light in space. Astronomers have been able to determine from geometrical considerations the paths of such objects as comets and meteors; it would therefore seem that the position of such an object as the Zodiacal Light ought ere this to have been determined.

Yet it must be admitted that there are peculiar difficulties in this problem. We can reason respecting the distance and motion of a comet, because we know that our observations are made on one and the same body, whose motions are in accordance with the laws of gravity. It is otherwise with respect to the Zodiacal Light. We see a certain glow or radiance occupying a definite position with respect to the horizon and to the celestial circles; but we have no means of ascertaining whether the objects from which that radiance proceeds are the same at any one time as at any other, or indeed (as will presently appear) whether a single one of the constituents forming the zodiacal gleam at one season is present within the same region of the solar system at another.

The geometrical considerations applicable to the Zodiacal

Light are, however, too definite to admit of question—in other words, the path to be followed in seeking for a theory of this object is unmistakable. Hitherto, so far as I am aware, that path has not been traced out *far enough* for the attainment of definite views—the perplexities which presently surround us as we follow it having seemed perhaps to render further research hopeless.

It happens, however, not unfrequently, that the very difficulties surrounding a subject of this sort assist us—in this way—that they enable us to reject theories which otherwise might engage our attention and so cause perplexity. Precisely as the very complexity of a lock makes us all the more certain that a key which opens the lock is the key really appertaining to it; so, where a subject of astronomical research presents many perplexing phenomena, these become so many reasons the more for accepting a theory which is not contradicted by any one of them.

This is, I think, the case with the Zodiacal Light. By considering the peculiarities of this object, we are able—as I hope now to show—to get rid, one after another, of various theories which might otherwise distract our attention. And though by this process of elimination we may not be enabled to determine quite the true theory of this object, we can yet considerably narrow the field within which selection has to be made.

The first considerations to be dealt with are those which depend on the normal features of the Zodiacal Light. It is well known that the light exhibits usually the figure of an oblique conoid whose axis lies close by the ecliptic, and whose vertex lies at a varying distance from the position of the Sun. Near the axis the light grows brighter, except close by the vertex, where it is even fainter than at the other parts of the border. The following table, prepared by Herr Klein, from modern observations, indicates the varying range of the vertex from the place of the Sun—though it must be remembered (and will be recognised at once by everyone familiar with the varying position of the ecliptic during the year, and other like circumstances) that these measures indicate variation in the extent of visibility rather than (of necessity) any real variation in the extent of the light.

Day of the Year		Distance of Vertex from Sun.	Part Observed.
January	2	83°·0	Western half.
	26	91·8	,,
February	11	81·0	,,
March	14	74·0	,,
April	14	75·0	,,
May	4	65·0	,,
August	1	77·0	,,
September	15	58·0	Eastern do.
October	17	74·5	,,
November	12	71·3	,,
	29	56·5	,,
December	13	61·0	,,
	28	80·5	,,

The setting of the Zodiacal Light when the western half is visible, and the rising of the light when the eastern half is visible, take place quite regularly, and in a manner precisely corresponding with what would be observed if the Zodiacal Light were a distant object like a planet, a star, or a portion of the Milky Way.

Now these circumstances at once enable us to reject the theory that the Zodiacal Light is a terrestrial appendage—by which I understand for the moment an object lying within the Earth's atmosphere. For there can be no question whatever that if any definite portion of our atmosphere were rendered luminous in any way, that portion would either occupy an unchanged position, or would shift according to the laws regulating the process of illumination, or according to the winds, or other like terrestrial causes. Now that on any given occasion such causes might so operate as to give the illuminated air the appearance of rising or setting as celestial objects do (that is, not *merely* rising or setting, but rising or setting along declination parallels) is quite possible, however unlikely. To take an illustrative instance: a balloon, seen at any one instant between an observer and the Sun, might be carried by the winds so as to continue between him and the Sun, even until the hour of sunset. But to suppose that night after night at any station a relation so peculiar would characterise the illuminated air, is like supposing that a balloon, started day after day from a given place, would day after day fulfil the condition considered above. This is obviously incredible. But even if it were credible, it would be insufficient, since the region of our atmosphere which would have to be illuminated in order to account for the Zodiacal Light as seen in one place, would, as seen from other stations, present an appearance wholly different from that of the Zodiacal Light. In fact, if the former place were in England, the Zodiacal Light would actually be overhead at places 900 miles or so west or east of England.

Next we have the normal aspect of the Zodiacal Light in different latitudes to consider. Now we have the most positive assurances from astronomers of eminence that the Zodiacal Light, wherever seen, occupies ordinarily precisely those regions of the heavens corresponding to the theory that it is too far from the Earth to have an appreciable parallactic displacement. We have the evidence of practised astronomers like the Astronomer Royal for Scotland, Captain Jacob, and others; and all the evidence we have points to the conclusion that the Zodiacal Light, as seen in the tropics, extends at any moment over those same parts of the stellar heavens which it illuminates as seen from our northern stand-point. The *limits* of the light may seem greater in those latitudes than in ours, but the axis of the conoidal gleam is situated precisely as with us.

Now it seems wholly unquestionable that this quality of the light should dispose at once and for ever of the theory that the Zodiacal Light is due to the existence of a ring of matter around the Earth.

Let it be remembered that there is only one way in which the ordinary aspect of the Zodiacal Light can at all be interpreted on such an hypothesis. If there were a ring of meteorites as far from us as the Moon is, then undoubtedly there would be a gleam in the west after sunset, and in the east before sunrise, in the position where we see the Zodiacal Light. And further, the individual meteorites producing any portion of that gleam would undoubtedly rise and set much as the Zodiacal Light is observed to do. But there would also be a gleam, and a much brighter gleam, in the south. The meteorites rising and setting would turn only a small portion of their illuminated faces towards us, those in the south (on or close by the ecliptic) would be 'full,' so to speak, and their combined lustre would be proportionately more considerable. Now supposing the ring exactly coincident with the ecliptic, the Earth's shadow would fall on the part due south. But the width of this shadow would (on the supposition we are considering) be relatively small. At midnight, in our latitudes, we should undoubtedly, on this supposition, see two arms of light extending from the eastern and western horizon along the ecliptic, each growing brighter and brighter towards the south; and a relatively narrow black rift would lie between the bright extremities of these arms. It is no theory that this would be the case, but a simple deduction from the most obvious geometrical laws.

If then we are to have a ring round the Earth, it must lie far within the Moon's orbit, so that the Earth's shadow may be wide enough to cover the meteorites along the whole of that long arc which under ordinary circumstances is undoubtedly unilluminated. The Earth's shadow cannot be more than 8,000 miles across *anywhere*, and we must have our ring at such a distance that this width of 8,000 miles may correspond to (or subtend) that wide arc of darkness actually observed under ordinary circumstances. (It is absolutely essential that ordinary circumstances should be accounted for; only when this has been done need we begin to inquire into extraordinary circumstances.)

Now we need not leave our own latitudes to decide how far off the ring should be to account for the apparent dimensions of the Zodiacal Light; because on the theory that the Earth's shadow, falling on a ring of some sort, defines the limits of visibility of the light, it would follow, precisely as in the case just considered, that the light would grow brighter and brighter up to the very edge of the shadow. (Supposing that edge to correspond to the extent of the Earth's shadow, there would be a somewhat ruddy bordering; but up to the commencement of that fringe there would be a regular increase of brilliancy.) But passing over this consideration (and also the consideration that the observed aspect of the Zodiacal Light in our latitudes is wholly inconsistent with the aspect thus shown to be due to the hypothesis we are dealing with), we may take as most favourable to the hypothesis of a meteoric ring near the Earth those observations of the Zodiacal Light in tropical regions which give to the ordinary apparitions of the light the greatest observed extension from the Sun.

We have it on the authority of Professor Piazzi Smyth that, even when he observed the Zodiacal Light under exceptionally favourable conditions—from an elevation, namely, of no less than 11,000 feet above the sea-level—the western tongue had completely set fully four hours before the eastern tongue began to rise. Now even if the eastern tongue were just beginning to rise when the western tongue had fully set, there would still be an arc of 180° between the two vertices.* But the shadow of the earth would not account for such an arc as this between the vertices, unless the outer part of the ring had a radius not exceeding $\sqrt{2} \times$ radius of the Earth (even in the most favourable case of a station near to the equator), and with such a radius as this the outer part (even) of the ring would be always invisible from places having a higher northerly or southerly latitude than 45°.

And even if we set this demonstration on one side for a moment, it is yet obvious that a ring lying relatively near the Earth, whether it coincided in plane with the equator, or with the ecliptic, or with any intermediate plane, could not possibly exhibit any approach to coincidence with the celestial ecliptic, when viewed from high latitudes. Further, as seen from high northern latitudes, such a ring would always have a parallactic displacement causing it to lie to the south of its geocentric position, and *vice versâ*: whereas no such association between the latitude of the observer and the apparent position of the Zodiacal Light has ever been observed; far less such a systematic association as the case requires.

It is geometrically impossible, then, that the ordinary aspect of the Zodiacal Light can be accounted for by any theory which represents it as due to a ring of light-reflecting bodies around the Earth, whether that ring be close by the Earth or at a distance comparable with the Moon's.†

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* It must be remembered that each vertex, as the Zodiacal Light was seen by Professor Smyth, lay close by the ecliptic.

† While dealing with the relations presented by the Saturnian ring-system, in 1864, I was led to apply the formulæ, with suitable changes of elements, to the case of a ring circling the Earth; being invited to the inquiry by the perusal of the observations made by Lieut. Jones, and comments made thereon by Baron Humboldt. I found that there is not a single hypothesis as to the dimensions of such a ring which would lead to results according with or even in the slightest degree approaching the results of observations made upon the Zodiacal Light. This conclusion is embodied in a note at p. 117 of that treatise;

We need not consider the theory that the light may be due to a self-luminous ring around the Earth, for obvious reasons.

Now, passing from the normal features of the ring to more or less exceptional peculiarities, we find ourselves compelled to reject yet one other theory of the light—I mean the theory that it is due to a disc of minute bodies travelling in orbits of small eccentricity around the Sun.

The peculiarities which oppose themselves most strikingly to this theory are those which relate to the position and extent of the Zodiacal Light, though it will be obvious that the observed variations in the apparent brightness of the light are not readily explicable on this hypothesis.

Admitting the existence of a disc of bodies, travelling as supposed, it will be evident that the changes affecting the motions of any member of the system would correspond exactly to those which would affect the motions of any considerable orb travelling at a similar distance from the Sun. In other words, the changes would resemble those slow periodic changes which affect the orbits of the Earth, Venus, and Mercury. Nor is it conceivable that the members of the system would so interfere with each other's motions as to affect appreciably at any time the appearance of the disc. Now changes such as these, affecting the individuals of a set of bodies which at any one time were spread with a certain uniformity (as the ordinary appearance of the Zodiacal Light would imply to be the case with its constituents) could not account for the observed changes in the position and extent of the light. The axis of the gleam has

in which note I remark that such investigations 'prove that the Zodiacal Light cannot be due to a ring of minute satellites surrounding the Earth, the appearance of the ring in high latitudes being altogether different from that which would be presented by a ring surrounding the Earth.' I am careful to refer to these researches and their results, because remarks have been published implying that I have somewhat hastily come to a decision on the points here dealt with. A complete mathematical investigation of the subject, made fully eight years since, may be regarded as fairly meeting those remarks.

been seen at times by practised observers, inclined at a considerable angle to the plane of the ecliptic. The extent ot the Zodiacal Light has varied at times in the most remarkable manner, while its luminosity has been so variable that sometimes for months together it has been scarcely perceptible (in our northern latitudes); while at others it has been singularly conspicuous. I set on one side for the moment those observations by Lieut. Jones which would imply that at times the Zodiacal Light increases so greatly in extent as to become visible at once both on the eastern and western horizon. I also set on one side those observations by M. Liais according to which the Zodiacal Light can be seen at times extending as a complete arch from the eastern to the western horizon. Assuming these observations to be reliable (and those by M. Liais do not seem open to question), a true theory of the Zodiacal Light may be expected to account for them. But without insisting on this, it is evident, I think, that the admitted variations of the Zodiacal Light, in position, extent, and splendour, do not admit of being interpreted by the theory that the light is due to a disc including always the same materials moving in orbits of small eccentricity.

Nor do our difficulties seem removed if we assume that the constituents of the disc travel in orbits of considerable eccentricity, so long as we suppose that the actual constitution of the disc is constant, or nearly so, amidst whatever variations in the distribution of individual constituents.

Yet the general aspect of the Zodiacal Light, and the considerations already applied to other theories, suffice to prove that there is always present around the Sun, as centre, a disc either composed of discrete meteorites, of vaporous masses, or of some combination of these and other forms of matter. The materials of this disc must be in motion around the Sun in accordance with the laws of gravity; at least we have no evidence whatever inviting us to the supposition that they differ in this respect from all the other constituents of the solar system.

We are thus led to the conclusion that the bodies composing the Zodiacal Light travel on orbits of considerable eccentricity, carrying them far beyond the limits of what we may now term the zodiacal disc. The constitution of the disc thus becomes variable, and that within limits which may be exceedingly wide. They must be so in fact, if all the recorded variations of the Zodiacal Light are to be accounted for. In other words, it is requisite (if our evidence is to be explained) that the paths of the materials comprising the Zodiacal Light shall be not only for the most part very eccentric, but that along those paths the materials should not be strewn in such a way that a given portion of any path is at all times occupied by a constant or nearly constant quantity of matter.

According to this view the constituents of the Zodiacal Light would—at least as respects distribution along their several paths and the general figure of those paths—resemble very closely the meteoric systems which, as we know, the Earth traverses in the course of her annual motion around the Sun.

By considering the Zodiacal Light we have thus been led to a theory involving, and associated with, the theory of meteor-systems as now established by the labours of Adams, Leverrier, Schiaparelli, and others. But it is worth noticing that by reversing the process, and considering first the theory of meteor-systems so established, we are led quite as readily to the theory that there must at all times exist in the Sun's neighbourhood a disc of discrete constituents which would present precisely such an appearance as the Zodiacal Light. I have shown elsewhere that this result is a simple mathematical deduction from the evidence.

But setting this consideration wholly on one side, the fact remains that all other theories of the Zodiacal Light—that is, of the motions of its constituent parts, without reference to its physical constitution—have been eliminated. It remains only to be shown that this theory is controverted by no peculiarities in the observed appearance of the Zodiacal Light, and also that we should inquire what further general laws, if any, may be predicated of the motions of the bodies composing this object.

The fact that the axis of the Zodiacal Light is ordinarily close to the ecliptic, is accounted for on the assumption that the various paths along which the constituents of the zodiacal disc travel, tend to aggregate towards the neighbourhood of the ecliptic. There is nothing, however, to prevent individual systems from having a considerable inclination to that plane.

The observed variation of the Zodiacal Light in brilliancy, position, and extent, is obviously to be expected according to the view of its structure now under consideration.

The simultaneous appearance of an eastern and western light and Liais's observation of a complete arch of light, have to be accounted for as highly exceptional, but at the same time recognised phenomena. It is easy to see that both these phenomena may be regarded as indicating the occasional but very exceptional extension of the zodiacal disc to a considerable distance beyond the orbit of the Earth. But it must not be concealed that there are grave difficulties to be removed before this interpretation can be regarded as satisfactory.

Let us suppose, for instance, the case of a thin luminous disc occupying the whole orbit of Mars, and that the Earth is in the part of her orbit where her distance north or south of this plane is greatest. Then it will be evident that the outline of the disc as seen from any part of the Earth would correspond very nearly to a great circle of the heavens, and that the whole of the visible heavens south or north of that great circle would be hidden by the luminous disc. In other words, a region of the heavens far larger than that occupied by the arch of Liais, or by the eastern and western lights of Jones, should be occupied by the Zodiacal Light if it had some such extension as we have assumed in the case of this luminous disc.

It is to be remembered, however, that, assuming (as we are bound to do) a considerable degree of flatness in the actual figure of the zodiacal disc, and more especially of its more distant portions, then much more light would be received from those parts towards which the line of sight is directed at a considerably acute angle, than from those parts which the line of sight crosses nearly at right angles. And it is easy to see that on any reasonable assumption as to the range of zodiacal substance which it is necessary that the line of sight should traverse in order that any appreciable light should be received, the occasional visibility of the light where the superior planets alone can be seen becomes as readily explicable as the ordinary visibility of the light in those parts of the sky where the inferior planets become visible.

It will be seen that all that can be strictly said to have been demonstrated in this paper is the fact that the Zodiacal Light is associated with the Sun, and not with the Earth; that it is not due to solar light reflected from bodies travelling within the Earth's orbit, whether in circular or elliptic orbits; and that if the major part of the Zodiacal Light is reflected solar light, then the paths of the bodies reflecting that light must resemble those of the meteors encountered by the Earth. As the spectroscope seems to show that at least a portion of the light* of the zodiacal gleam is not reflected solar light, we cannot, in the present state of our knowledge, definitely decide on a theory as to the motions of the bodies to which the light is due. For the solution of the problem is obviously bound up with the interpretation of the physical nature of the Zodiacal Light. If some solar action, for example, rouses luminosity in certain definite directions—as, for instance, near the plane of the Sun's equator—in some such way as light is caused to appear along radial lines through and beyond the heads of comets, our power of theorising from such considerations as have been dealt with in this paper would be limited. It would still remain certain that the Zodiacal Light is not a terrestrial appendage (either near or far off), but what sort of solar appendage it might be would be a problem as difficult to solve as that presented by comets.

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* I use this mode of speaking not by any means as doubting the accuracy of Ångström's observation; but because even if the greater part of the light gave

If the radiated structure of the Sun's corona as seen under favourable atmospheric conditions should be confirmed as more than an optical phenomenon, it is not impossible that we might be put in the way of interpreting the Zodiacal Light.

Monthly Notices of the Royal Astronomical Society for Nov. 1870.

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a continuous spectrum, yet this spectrum might remain undiscernible even when bright lines corresponding to a very minute proportion of the total light were seen with ease. Nay, such bright lines as Ångström found in the spectrum of the phosphorescent light from the sky might be detected when a continuous spectrum from the much brighter light of the zodiacal radiance remained unseen.

THE SOLAR CORONA AND THE ZODIACAL LIGHT

WITH SUGGESTIONS RESPECTING OBSERVATIONS TO BE MADE DURING TOTAL SOLAR ECLIPSES.

(A PAPER WRITTEN WITH SPECIAL REFERENCE TO THE ECLIPSE OF DECEMBER 1870.)

TOTAL eclipses of the Sun last so short a time that, if possible, no part of that time should be wasted through a misapprehension of the nature of the phenomena to be observed. On this account I cannot but think it would be a matter to be much regretted if mistaken views were promulgated respecting the corona, supposing it to be possible—which I take to be the case—to form just views from the evidence already in our hands.

The principal object of the observations to be made during future solar eclipses will be to ascertain the characteristics of the solar corona. Observers will certainly be able to work much more effectually if they know beforehand the general nature of the phenomenon, for they will thus be guided not only in the selection of modes of observation, but also by knowing what points it is most important they should attend to.

I think it so essential to avoid raising unnecessary doubts, that I would not venture to express the opinion that the corona is wholly a solar appendage if I had not given the matter very careful consideration, and found the evidence overwhelmingly strong in favour of this view.

It is hardly necessary to discuss the theory that the corona is due to the diffraction of solar rays which pass near the Moon's edge, because that theory has been thoroughly disposed of by Brewster's arguments. Nor need we consider La Hire's theory that the phenomenon is due to the reflection of the solar rays from the irregularities of the Moon's surface, as it is obviously inconsistent with the observed peculiarities of the corona.

But a theory has recently been put forward that the corona is simply due to the glare of the terrestrial atmosphere, and this theory has been adopted by astronomers of standing. I hold it to be important, therefore, that this theory should be subjected to careful scrutiny, as undoubtedly, if it be erroneous, much mischief may be done to the cause of scientific progress by its promulgation.

The first and most obvious evidence against this theory is the fact that the Moon is projected as a dark disc on the bright background (so to speak) of the corona. The theory requires that the corona should, in fact, not be a background, but a foreground; and one might naturally inquire how the Moon, which is beyond the Earth's atmosphere, should come to be apparently projected upon the supposed glare of that atmosphere.

But though this circumstance is in itself decisive of the matter at issue, let us turn to less obvious considerations. As a matter of fact, we know that light reaches the eye along lines tending from the neighbourhood of the eclipsed Sun. Let us inquire whether in those directions there is illuminated air; if not, optical considerations will force us to regard the source of light as beyond the air.

The eclipse of December [1870] is not a favourable one for my argument; but it will be more interesting, and perhaps more useful, to consider it than any other.

In Fig. 8, let A represent the position of an observer on the line of central eclipse, somewhere in the south of Spain. At such a station the eclipsed Sun will be almost 30 degrees above the horizon; and I find from a valuable paper which Mr. Hind has been good enough to forward to me, that the shadow-cone will be about 50 miles across, where it reaches the Earth. Obviously, then, the shadow on the Earth will be an ellipse whose major axis will be about 100 miles, its minor about 50 miles in length. Let A S then be drawn inclined at an angle of about 30° to H A H′, the horizon line at A in a vertical plane through the Sun; and,

Fig. 8.

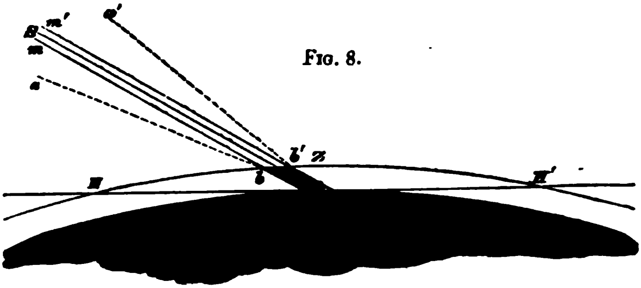
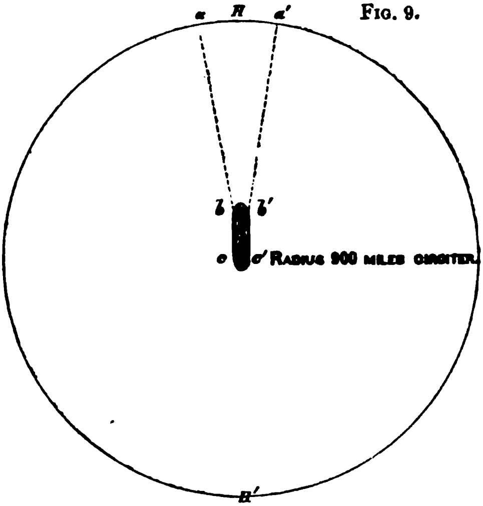


Fig. 9.



having A *c*, A *c'* each to represent a space of 50 miles; let *c m* and *c' m'* be drawn, each inclined about 16' to A S, so that while A S is directed towards the Sun, *c' m'* and *c m* would be directed towards the highest and lowest points of the Moon's limb. Then *m' c' m c* is a vertical section of the Moon's shadow.

Now we do not know the height of the terrestrial atmosphere, but we may confidently believe that no air above the height of 100 miles can reflect any appreciable amount of solar light to us.* Let us therefore take A Z to represent 100 miles; then H Z H' will represent the limits of the light-reflecting air, where H H' is about eighteen times as great as A Z. The portion of the atmosphere above the horizon-plane of the observer will therefore be of the figure produced by the revolution of H Z H', about the vertical axis A Z. It will be, in fact, a plano-convex lens.

Let *c m* and *c' m'* meet H Z H' in *b* and *b'*; then the portion *b c c' b'* will be in the Moon's shadow. (The effects of refraction are obviously insignificant.) The only light which can reach this part of the atmosphere is that from the chromatosphere (to use a convenient but unsatisfactory name) and the coloured prominences, or from the earth and surrounding illuminated air. Towards *b* and *b'* the observer will recognise the first faint traces of directly illuminated atmosphere, and the light will gradually increase above *b'* and below *b* (more rapidly in the latter case than in the former). By a careful construction (a method quite exact enough for such an inquiry as the present), I make the angle S A *a* about 6°, and the angle S A *a′* about 9°.

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* Bravais, from a discussion of Lambert's observations of the crepuscular curve, deduced a height of nearly 100 miles. His own observations, made from the summit of the Faulhorn, gave a height of about 66 miles. Neither estimate refers to the actual limits of the atmosphere however. Dr. Balfour Stewart considers that perhaps the best means of judging on this point would be by observations made on the aurora. From such observations made in 1819, Dalton estimated the extreme height of the auroral light at 102 miles; Sir John Herschel estimated the height of an auroral arch seen on March 9, 1861, at 83 miles (undoubtedly the aurora is often seen much lower). The limits of air capable of reflecting light must certainly lie much below the actual limits of the terrestrial atmosphere.

This, however, refers to only one section of the shadow-cone. To determine (roughly) the extent of illuminated atmosphere in a horizontal direction, we have only to consider the air-lens H Z H′ as supposed to be viewed from above. In Fig. 9, *c c′* represents the actual shadow on the earth; *b b′* the intersection of the shadow-cone with the limits of our hypothetical envelope 100 miles high. Thus *b b′ c′c*, Fig. 9, represents simply a vertical view of the portion *b c c′ b′* of the shadow-cone in Fig. 8. Lines *b a*, *b′ a′*, drawn from the centre of the ellipse *c c′*, touching the ellipse *b b′*, give approximately the angular width of that part of the heavens within which no atmosphere directly illuminated by the Sun can be visible. I find from a careful construction that *a b* and *a′ b′* would include an angle of about 14½°.

Thus we obtain a nearly circular region (in which the Sun is eccentrically situated), having a horizontal diameter of about 14½°, and a vertical one of about 15°, within which there is not any light whatever from directly illuminated air. The Sun would be about 6° from the lowest point of this dark region.

With regard to the light from the prominences and the chromatosphere, upon the air within this region, we know that it cannot suffice to light up the air with any strong, if even with any appreciable glow; because we know how small a relation ordinary atmospheric glare bears to direct solar light, and the glare due to the chromatosphere and prominences would bear a similar relation to the direct light from those sources. But further, whatever light came in this way would obviously illumine the outer parts of the shadow-frustum $b\,b'\,c'\,c$ more strongly than the parts near the axial line A S. Hence a faint diffused light diminishing *towards* the neighbourhood of the Moon should result.*

As regards the illumination of the shadow-frustum by light derived from the neighbouring illuminated atmosphere and from the Earth, it is only necessary to remark that even when there is no eclipse the light thus falling on such a region as *b b′ c′ c* would be small; but that while a total eclipse is in progress all the parts near the shadow-cone are in nearly total eclipse, and not any part of the whole region H Z H′ is illuminated by so much as half the solar disc. Further, the light derived from this source, like that derived from the prominences and chromatosphere, should diminish towards the neighbourhood of the Moon's disc, instead of increasing as the coronal light does. Also, the light from all these sources should extend over the Moon's disc, since it would illuminate the air between the observer and the Moon's body.*

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* This was confirmed during the eclipse of December 1870. Indeed, the light received from the direction of the Moon gave the 'prominence spectrum,' very faint, of course. It is noteworthy, however, that during the late eclipse the Moon's disc appeared green. It was compared by one observer to dark green velvet. At first sight it might appear as though a full explanation of this was supplied by the fact that the air towards the Moon's body was illuminated by the corona, the principal line in whose spectrum is green. But as the corona itself did not appear green, we must suppose that the chief portion of the corona's light was in reality that which gave the faint continuous spectrum; and this must needs be the case with the coronal light reflected by our own atmosphere. It may be assumed, therefore, that the coronal (reflected) light received from the direction of the Moon's body could not have been appreciably green; and the observed greenness of the Moon's disc must be otherwise explained.

It seems to me that a sufficient explanation is to be found in the nature of the light received by the Moon from the Earth during the eclipse. This light as respects quantity must have been considerable—in fact (for equal surfaces) some thirteen times that with which the full Moon illuminates the Earth. The Moon's shadow on the Earth would have the effect of diminishing this light by the same amount as if, instead of umbra and penumbra, there were a black shadow whose foreshortened aspect seen from the Moon equalled the Moon's disc as seen by ourselves. Its colour must have been green, I think; because the proportion of land and sea surface in the terrestrial disc, as seen from the Moon, was such that, calling the ocean blue-green and the land brownish (on the average), the resulting mixed colour would be a delicate olive green. (In the *Quarterly Journal of Science* for October 1870, I have shown the exact orthographic presentation of the Earth's disc towards the Moon near the epoch of totality.)

Now during the eclipse of 1860 land and sea were turned towards the Moon in different proportions. The eclipse occurred in summer, so that the northern or land portion of the Earth was less foreshortened, and furthermore the eclipse occurred at about three in the afternoon, by which hour the two Americas were well advanced upon the Earth's disc as seen from the Moon. One would therefore expect that the Moon's disc on that occasion should have presented a brown hue. And accordingly we find Mr. De La Rue so describing it.

It would appear probable, therefore, that our Earth, as seen from distant stations, as Venus or Mercury, is usually a green planet, but sometimes dun or fawn-coloured. Also her rotation may probably be recognised from Venus without telescopic aid, simply by her colour-changes.

It follows then that, so far from giving an account of the corona, atmospheric glare gives us a dark region round the eclipsed Sun, and a gradual increase of light with distance from him.

Within this dark space the disc of the Moon, illuminated by the Earth with about thirteen times as much light as the new Moon sends to us, ought to be conspicuous by its relative brightness.

Now, though the reasoning here deals with relations so simple that a mistake can hardly arise, yet there are certain tests to which these conclusions may be submitted before we proceed.

It is clear from Fig. 8, that *before* the limits of the total shadow reached A there *should* be atmospheric glare towards the Sun, and further that this glare should at first wholly cover the Moon, and rapidly sweeping across her disc, just before totality, should pass away from her neighbourhood with undiminished velocity. It would be difficult to detect such a phenomenon by ordinary observation; though, as I shall presently show, not impracticable. But supposing a photograph could be taken an instant before totality, we might catch the glare while in the act of crossing the Moon's disc. Now this could only be managed by a miracle of dexterity; but, by a miracle of good fortune, it *has* been managed already. The first photograph of Lieut.-Col. Tennant's admirable series was taken an instant before totality commenced; and *there we have the glare just about to leave the Moon's disc, but still trenching most obviously upon it.* Fig. 10 represents the feature here dealt with. The light here is true atmospheric glare, and we see that, as might have been thought obvious, it is not limited by the Moon's disc which lies so far beyond the limits of the air. Then also we notice another important point. The edge of the glare is obviously travelling much faster than the Moon; for while the Moon proceeds to obliterate the last remaining point of the Sun's disc, the glare traverses the much wider distance separating its inner edge from the Moon's limb. Clearly this velocity would carry the glare clean away from the Moon, as the above reasoning shows should be the case.

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* It will be perfectly obvious that the line of reasoning here adopted involves the conclusion that if the corona be a solar appendage, there will be an atmospheric glow due to the corona, as well as that due to the prominences and chromatosphere. But as my object was to prove that the corona is a solar appendage, I could not *here* speak of the effects due to this solar corona.

FIG. 10.

Again, it will be obvious, from a study of Figs. 8 and 9, that during an annular eclipse, at the moment when the shadow-cone is pointing directly, or almost directly, towards the observer, the centre of the Moon's disc ought to be much darker than the edge. Now in the tenth volume of our 'Memoirs,' Mr. Baily states that, while observing the eclipse of 1836, he noticed, on looking at the Moon through a telescope during the annularity, that 'the circumference was tinged with a reddish purple colour which extended over the whole disc, but increased in density of colour according to the proximity to the centre, so as to be in that part nearly black.' It is obvious that this appearance could last but a few seconds, since the moment the axis of the shadow-cone was turned appreciably away from the observer (and the vertex of the cone travels fully twenty miles per minute), he would be looking through the cone's *sides.* The following passage from Klein's *Sonnensystem* describes the whole phenomenon precisely in accordance with this view: 'Bei der ringförmigen Finsterniss, am 30. October, 1864, sah Mouchez zu San Catharina in Brasilien, im Augenblicke als die Scheiben von Sonne und Mond concentrisch waren, das Centrum des Mondes völlig dunkel, aber von hieraus gegen dem Rand nahm die Helligkeit regelmässig zu und letzterer erschien heller, oder doch wenigstens eben so hell, als das aschgraue Licht der Mondsichel, kurze Zeit vor oder nach dem Neumonde. Die ganze Erscheinung verschwand und die Mondscheibe war gleichförmig dunkel, als der leuchtende Ring gerissen und die Mitte der Finsterniss vorüber war.'

Taking the corona to be a solar appendage, it is clear that even in total eclipses a somewhat similar appearance might be looked for, the outer parts of the Moon's disc during central totality seeming brighter than the centre, because the atmosphere between us and those parts would be more fully lighted up by the corona. I find, accordingly, that M. Tissel, observing the total eclipse of 1733 at Skepshat, in Sweden, saw the Moon's surface brighter at the margin, and black towards the middle. We see from this most clearly that the atmospheric glare in this region is very much fainter than the corona; for, except on a very close examination, the Moon's disc, though the glare appears over a part of it during the totality, seems absolutely black, and is so rendered in photographs.

But further, if the views expressed above are correct, it ought to be possible, under favourable circumstances, to see the Moon's face by reflected earth-light. I find that Bigerus Vassenius, during the remarkable total eclipse of 1733, using a telescope of 21 feet focal length, perceived the principal spots on the Moon during the total obscuration (*Phil. Trans.* 1733, p. 135). Ferrer also saw the spots on the Moon's surface very plainly during the total solar eclipse of 1806.

Yet again, if the apparent blackness of the Moon's disc results from the fact that the coronal light is beyond the Moon, and so forms the background on which she is projected,* two phenomena might be expected to be visible under favourable circumstances. First, the entire outline of the Moon's disc ought to be visible in partial eclipses, or before and after totality; and secondly, the corona ought to be visible at such times, and also during annular eclipses. I find that the former phenomenon, which corresponds in reality to the visibility of the corona (since were there no corona the Moon's limb could not appear dark where it crossed the Sun), has been frequently noticed; it has, in fact, been as often recognised as looked for. The visibility of the corona, when the Sun is not totally eclipsed, has also been so frequently recognised that it is hardly worth while to mention instances in point.† But I may quote, as very remarkable, the fact that in 1860 Father Secchi saw the corona *for forty seconds after totality was past.* Another remarkable instance is that recorded by M. Edstrom, in the case of the eclipse of 1733, when the unequal radiations of the corona were observed to remain unchanged in position, as they gradually faded out of view with the increasing solar light.

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* The fact that the disc of Venus appears blacker than the surrounding sky when she is in superior conjunction, can only be explained by supposing there is some light beyond Venus. What can that light be but a solar appendage?

† Arago has founded on the visibility of the corona while a portion of the Sun is yet uneclipsed, a calculation of the ratio in which the coronal light exceeds that of the atmospheric glare *then* undoubtedly present. That the corona is brighter than the atmospheric glare caused by a portion of the direct solar light undoubtedly follows from the visibility of the corona under such circumstances; but Arago's mode of treating the problem is not exact. He makes the atmospheric glare proportional to the portion of the solar disc

It is further obvious that if the corona be a solar appendage, one would expect it to appear concentric with the Moon only at the moment of central eclipse. Now I find numerous instances in which it has been stated that, quite obviously, the widest part of the corona was first on the side the Moon had just covered before totality, and lastly on the side she was just about to leave uncovered. I also find several statements (one or two very positive) that the corona was centrally disposed round the *Moon* throughout the totality. I would remark on this, that observations of the former kind, besides being more numerous, are severally more effective than observations of the latter kind. For the former refer to the recognition of a phenomenon and afford positive evidence; the latter merely assert the non-recognition of the phenomenon, and supply therefore only negative evidence. The former describe a peculiarity which attracted the notice of observers; the latter may be taken quite as well to indicate a want of skill in observation as the non-appearance of the particular phenomenon in question. All the positive evidence is therefore here also in favour of the view that the corona is a solar appendage.*

It remains that I should touch on other evidence we have

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visible at the moment. In reality, this proportion does not hold, for the upper regions of the air are illuminated by much more of the Sun's disc at such a time. In fact, the problem is one of much greater complexity than Arago seems to have imagined.

* The centricity of the corona concerns, however, the question whether the corona is a solar or a lunar appendage; since the atmospheric glare should shift even much more obviously with respect to the Moon than a solar appendage would seem to do.

of the existence of a solar appendage adequate to produce the observed appearances.

And first let us consider the zodiacal light. We know that even in our latitudes this phenomenon often exhibits a remarkable degree of luminosity towards the horizon and near the core of the gleam (so to speak). But in tropical countries the brightness of the zodiacal light is much more striking, and is seen to grow visibly greater in the Sun's neighbourhood. At heights of from 8,000 to 12,000 feet in tropical climates, says Humboldt, the zodiacal light is seen of a brightness exceeding that of the Milky Way between Aquila and Cygnus. And obviously if we could trace the zodiacal light up to the solar limb, we should see it shining with a glory far exceeding that which it shows even in tropical countries. For we know that the brightest part there seen belongs still but to the outskirts of the object.*

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* I ought, perhaps, to show reason for regarding the zodiacal light as a solar appendage, notwithstanding Dr. Balfour Stewart's recent suggestion that it may be a terrestrial phenomenon. But in reality there can be no doubt whatever that the zodiacal light cannot be a phenomenon associated in any way with our atmosphere. Doubtless Dr. Stewart, whose mathematical attainments are well known, must have directed his attention too exclusively to the physical requirements of his theory, or he would not have overlooked obvious mathematical objections against it. The portion of the return-trade region above the horizon of any place is clearly a lamina shaped like a watch glass (slightly convex, see fig. 8), and the whole of this should be illuminated by electrical discharges excited in the way he suggests. We may, in fact, see in this an explanation of the familiar phosphorescence seen sometimes to cover the whole heavens (which gives the same spectrum as the aurora and zodiacal light); and even though at times, or even commonly, only a portion of this lamina should be so illuminated, no reason can be shown why that portion should always be an inclined tongue-shaped slip, as it should be, to account for the zodiacal light in our latitudes. It is hardly necessary, however, to point out to the astronomical reader that a light which exhibits no parallactic displacement, which varies in position for different latitudes according to the laws affecting the celestial bodies, which rises and sets according to the same laws, and which lastly affects the neighbourhood of the ecliptic, cannot by any possibility belong to the Earth's atmosphere. The zodiacal light *might* be explained as due to a ring of matter surrounding the Earth, at a distance nearly equalling the Moon's, and travelling (as such a ring would) nearly in the plane of the ecliptic. Such an explanation was indeed put forward in 1856 by Prof. Heis. But the

Hence we should expect to find precisely such a glow of light round the Sun in total solar eclipses as we actually do see.

Again, from what we now know respecting meteors, we may derive abundant evidence in favour of the view that countless myriads of these bodies must always lie in the Sun's neighbourhood. For though, while the meteoric orbits were supposed circular, there was nothing very surprising in the fact that the Earth encountered more than a hundred meteor systems (because the zone of such systems seemed to lie close by the Earth's orbit), yet now we know how eccentric the meteoric orbits really are, we recognise the fact that antecedent probabilities would be wholly against the Earth's encountering even one such system, were there not many millions of them. And since she encounters more than a hundred, we conclude that there must be millions on millions of such systems having their perihelia within the Earth's orbit. These uncounted systems ought to become visible during a total eclipse, since their dispersed members would lie in all directions round the Sun. Those meteoric flights also which were near him (and many must pass very near to him) would shine with a light whose brilliancy would go far to make up for the extreme relative minuteness of the individual meteors. Since near the Sun's disc the line of sight would be directed through a range of many millions of miles over which such meteors must be freely distributed, while along some 200 millions of miles in this direction meteors must be scattered

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phenomena of the zodiacal light are much better explained by the theory that it is due to a solar appendage, even if we admit that the light sometimes extends from the eastern to the western horizon. But while Heis' theory, with overwhelming probabilities against it, has some points in its favour, the theory that the zodiacal light is an atmospheric phenomenon, is absolutely untenable. If anything could render the theory more strikingly opposed to observation than it is, it would be those occasional peculiarities of the zodiacal light which have been thought by some to favour the theory. These peculiarities simply add new difficulties to others already overwhelming.

more or less richly, one can recognise the reason of the brightness of the corona near the chromatosphere.

Further, we know from the researches of Leverrier that there must exist continually in the Sun's neighbourhood a quantity of matter sufficiently important to affect the motion of the perihelion of Mercury. A few relatively considerable planets (as large, say, as the asteroids) might effect the observed changes; but far more probably a multitude of minute bodies may be held to be in question. Now the constant presence of meteors in the Sun's neighbourhood would produce the observed results, even though the individual meteors might remain but a brief time in the Sun's neighbourhood, to pass away presently on orbits whose aphelia might lie far beyond the orbits of the major planets.

Further, Mr. Baxendell has shown that certain peculiarities of magnetic and thermal change seem to point very decisively to the existence of a solar appendage holding the position which the corona, regarded as solar, seems to occupy. I have had the pleasure of discussing with him many of the relations considered above, and I find that there is nothing in his valuable meteorological researches which opposes itself to that particular view of the corona which I have advocated above, while his main result (which I hold to be of extreme importance) supplies an obvious argument in favour of that view.

Lastly, there are certain peculiarities in the aspect of the corona which seem only explicable on the theory above enunciated. Such are those radiations which are not at right angles to the Sun's limb; the phenomenon of loops of light in the corona with their concavity directed towards the Sun; the strange appearance resembling a hank of thread in disorder, seen by Arago in 1842; and other peculiarities too numerous to specify.

I know not of any phenomena which oppose themselves to the view here put forward, though I have carefully sought for such.

The spectroscopic analysis of the corona has not hitherto been altogether satisfactory, so that it may hardly be well to lay much stress upon it. It accords very satisfactorily, however, with the above theory. There would be a large quantity of reflected solar light in the corona, but there would also be much light from incandescent meteors, since those which came within a million miles, or so, of the Sun would undoubtedly be raised to a white heat. Some of the meteors would, in all probability, be vapourised, and so a portion of the light they supply would give a bright line spectrum, though probably of extreme faintness. The observed association between meteors and comets suggests obvious considerations in explanation of the peculiarities which characterise the spectrum of the corona. If the great comet of 1843, which passed within 65,000 miles of the Sun, has, like Tempel's comet, a train of meteoric bodies following in its track, these must be vapourised in the Sun's neighbourhood.

The contradictory evidence afforded by the polariscope is also obviously accounted for by the theory I have here advocated, even if it may not be said of itself to force upon us the belief that the light of the corona is of that mixed kind which could scarcely result but in the way specified in that theory.

It would be desirable that measures should be adopted to insure the application of effective modes of observation during the very brief interval of total obscuration. I think the Astronomical Society might with advantage appoint a committee to consider whether novel appliances and methods might not be employed to good purpose. The points I now proceed to touch on are so simple that some apology may, perhaps, be needed for bringing them under the notice of the

Society; but if they should lead practised observers to make really important suggestions, my purpose will have been fulfilled.

In the first place, I would remark that observations specially directed to prove that the corona is a solar appendage would, in my opinion, be a complete waste of time and skill. It would be a misfortune—nay, it would even be in a sense discreditable—to astronomy, if the attention of observers should be directed to the solution of a question which has been practically solved during former eclipses. Unless the most obvious considerations of mathematics and optics are to be entirely neglected, the position of the corona as a solar appendage must be regarded as established, and all observations made with the object of confirming or disproving the matter, as simply futile.

But if we must travel over old paths, in order to make plain that which is already demonstrated, there are a few modes of observation which may be suggested as likely to give significant, however unnecessary, evidence.

If an observer were to confine his whole attention to the lunar disc during the eclipse, having a telescope with well-adjusted clock movement, and a field somewhat less than that of the full Moon, he would be able to recognise the following striking proofs of the real way in which the glare of the atmosphere varies during an eclipse. He would see, as the total phase approached, the atmospheric glare over the Moon's face gradually diminished, and then what remained of actual glare from direct solar rays sweeping rapidly across the face of the Moon and leaving her disc relatively dark. But in a few moments the observer would be able (in favourable atmospheric circumstances) to recognise the spots on the lunar surface.

If an observer were to limit his attention to the Moon's disc during totality, keeping his eyes in darkness until the commencement of totality was announced by those around him, he would be certainly able to see the lunar spots, unless atmospheric conditions were very unfavourable indeed.

Attention might be directed to the shape and motions of the dark region of the sky surrounding the corona; and such observations would not be so complete a waste of time as those last considered, since it is evident that important information might be gathered from them respecting the height of the atmosphere. Such information would be in many respects more trustworthy than that which has been derived from the position, shape, and motions of the crepuscular curve.

But a mode of observation which I would advocate with great earnestness, is the simple application of telescopic power to determine, if possible, the structure of the corona. I have no doubt that this structure is continually changing; but most valuable information might be gained from a careful study of the position of the coronal beams at the time, and of those singularly complex hanks and streamers which have been already noticed by astronomers. The use of a telescope of low magnifying power, but of first-rate definition, a comet eye-piece being employed, would be desirable in thus studying the corona. The telescope should be accurately driven by clockwork, and a dark iris-disc, if I may so describe an arrangement which would be the converse of an iris diaphragm, might be employed with advantage to hide the light of the prominences and chromatosphere. If the field of view were several degrees in diameter, and the dark disc at the beginning of totality concealed a circular space extending a degree or so beyond the eclipsed sun, the observer might first examine with great advantage the outer parts of the corona, and gradually extend his scrutiny to the very neighbourhood of the prominences. Supposing his eyes had been kept in darkness before totality began, he would be able to gain such an insight into the real structure of the corona as has never yet been obtained by astronomers.

As regards the spectroscopic and polariscopic analysis of the corona I shall say little. It would obviously be most desirable that Dr. Huggins, Mr. Lockyer, and those astronomers whose attention has been practically directed to researches of this sort, should give careful consideration to the question how the short interval of totality may best be employed, and that they should make their views public as early as possible. To one point, however, I shall venture to direct the attention of observers. It seems to me most important that every observer proposing to take part in applying such delicate light-tests to the corona, should prepare for the observations he is to make by keeping his eyes in darkness as nearly complete as possible for some time before totality commences; and, further, where different parts of the corona are to be examined, the fainter parts should be first dealt with.

If the search for an intra-Mercurial planet is to be renewed with any chance of success, there can be little doubt that the telescopist must keep the corona, or at least its brightest portions, out of the field of view. A telescope specially constructed for the purpose, having a motion carrying the tube conically round a mean position might easily be devised; and with such an instrument one might conveniently sweep the Sun's neighbourhood all round the limits of the corona, for Vulcan and perhaps a train of attendant Cyclopes. But a telescope of low power, with a comet eye-piece, and a diaphragm concealing the brighter part of the corona, would probably be quite as effective.

For this class of observation also it would be very advantageous that the eyes should be kept in darkness for some time before totality commenced.

Are observers to be found who would be ready to forego the opportunity of witnessing one of the grandest of all natural phenomena, of watching the gathering shadows, of beholding the wonderful transformation of the face of nature, the weird and unearthly aspect of all things round them, and the strange beauty of the solar corona of glory, in order that they may devote all their observing energies during two short minutes to important, but severally uninteresting, phenomena? We know that, so far as the period of totality is concerned, such a sacrifice has already been made by De La Rue and Tennant, by Secchi, Janssen, Captain Herschel, Young, and a number of other lovers of science; but no observer has yet foregone the whole spectacle of a total eclipse for the sake of the dull, dry details of scientific observation.

Monthly Notices of the Royal Astronomical Society for March 1870.

FURTHER REMARKS ON THE CORONA.

At the meeting of the Astronomical Society in April 1870, Dr. Gould, of America, indicated his belief that the trapezoidal corona seen by himself and other observers during the progress of the American eclipse was in fact but the chromatosphere seen under unusually favourable circumstances. He added, that the light outside that four-cornered corona appeared to shift in position, and hence he concluded that it was terrestrial.

It seems to me that, if this view be admitted, the difficulty pointed out by Mr. Lockyer in the case of the corona considered generally, exists in scarcely diminished extent in the case of this trapezoidal appendage also. Estimated by most of the observers as extending fully 12′ from the disc of the eclipsed Sun, its real depth would be far more than 320,000 * miles, and the pressure even at the summits of the highest prominences would be enormous.

We gain nothing, then, by Dr. Gould's supposition; though of course that does not prove it to be erroneous. But Dr. Curtis (whose successful photographs appear in Commodore Sands's reports of the total eclipse) remarks that he has read Dr. Gould's statements respecting the eclipse with considerable surprise. After referring to the photographic evidence, he adds, 'Dr. Gould adduces as an additional argument in favour of his assumption the observation that the long coronal beams appeared to him to be 'variable,' while the 'aureole' photographed was evidently 'constant' during the time of totality. This argument, however, loses some of its force when it is remembered that to other observers the corona appeared to the eye absolutely unchangeable, both in form and position, during the whole period of the total obscuration.' He goes on to indicate the probability that Dr. Gould has mistaken a photographic effect for a real phenomenon, in this case, precisely as when he interpreted the apparent encroachment of the bases of the prominences on the Moon (a dark-room phenomenon, as Curtis shows) to 'specular reflection' at the Moon's surface.

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* Even in Mr. Whipple's photograph it has an extent of fully 6′, which would correspond to more than 160,000 miles, or 80,000 miles above the highest prominences yet seen.

I must confess that, after a very careful study of the whole series of American observations, Dr. Gould's view appears to me to be altogether disposed of by the concurrent testimony of so many and such skilful observers.

One striking, and as yet unnoticed, piece of evidence exists in General Myers' report of the appearance of the corona as seen from the summit of White Top Mountain, 5,530 feet above the sea-level. Here the same quadrangular aspect was observed as at lower levels (and in Whipple's photograph), but the rays were much longer. 'The silvery rays,' he says, 'were longest and most prominent at four points of the circumference—two upon the upper, and two upon the lower portion—apparently equidistant from each other, and at about the junctions of the quadrants designated as "limbs," giving the spectacle a quadrilateral shape.' He remarks that these silvery rays were 'straight and massive,' and extended 'to a distance of two or three diameters of the lunar disc.' He adds, 'There was no motion of the rays.'

It seems impossible to mistake the significance of these observations.

In the preceding paper on the corona and zodiacal light, I dealt specially with the theory that the corona is due to the illumination of the earth's atmosphere by light not affected 'by any action at the Moon.' Many of the arguments, however, apply equally well on the supposition that there is such action. The striking fact that at the time of central eclipse the cone within our atmosphere bounded by lines from the observer's eye to the Moon's limb, contains no light, while the cylinder within our atmosphere bounded by lines from the Sun's limb to (and produced beyond) the Moon's, contains much light, affords, I take it, absolutely convincing evidence that this light is derived from an object far beyond the Moon. For if we suppose the solar rays to get by any process within the cylinder, they should clearly traverse the cone also. For example, assuming that a solar ray passing by the Moon's edge is deflected (by whatever cause) so as to fall within that cylinder, into which (from its very nature) undeflected rays cannot pass, the deflection, in order to account for observed appearances, must carry the illumination of our atmosphere up to the above-mentioned cone, and there suddenly the illumination must cease. But the cone has no existence in nature; it is but a mathematical conception: why then should these deflected rays respect it?*

Even La Hire's theory, which De Lisle is supposed to have overthrown, seems more easily supported than one which requires a moving shadow-cylinder of air to be illuminated, while a fixed cone (*not* a shadow cone) within it remains in darkness.

It seems much more natural to regard the darkness of the lunar disc, and the relative brightness of the corona, as due simply to the fact that the Moon is an opaque body very much nearer to us than the corona.

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* Mr. Lockyer tells me that M. Faye expressly suggests that there is some action at the Moon, and that, according to his and M. Faye's theory, it is thus the atmosphere gets illuminated. What the nature of the action may be I have not yet heard, nor can I conceive of any which would account, however roughly, for observed facts.

Let me renew my statement that it is the importance of the approaching eclipse which forces me to urge now views which I have long entertained. It appears to me that if, as I hold to be the case, the evidence respecting the corona is amply sufficient to prove it to be a solar appendage, then it would be a serious misfortune if any observers were to devote their time to establishing this fact. Instead of this, I should be glad to see every moment of the short duration of totality devoted both by general observers and spectroscopists to the inquiry *what sort of a solar appendage* the corona may be. On this inquiry depend issues of the utmost interest and importance to science; the other would be a waste of time: on one question we have abundant evidence; on the other (to quote the just words of Professor Pritchard), 'wise astronomers profess their profound ignorance.'

Monthly Notices of the Royal Astronomical Society, October 1870.

NOTE ON OUDEMANN'S THEORY OF THE CORONAL RADIATIONS.

THE papers read at the meeting of the Astronomical Society, in March 1871, on the subject of the recent eclipse were so full of interest that it seemed desirable not to prolong a discussion which was raised respecting Oudemann's theory of the coronal beams. But as some of the results which the eclipse observers obtained would appear to be invalidated if certain points of Oudemann's theory were admitted, I venture briefly to note two objections of which the second at least seems decisive against those points.

Admitting, with Oudemann, the probability that the inter-planetary spaces are occupied, to distances from the Sun far exceeding the radius of the Earth's orbit, by matter capable of reflecting a certain proportion of light, it yet appears improbable that the whole quantity of such matter within a distance from the Earth equal to the radius of the Moon's orbit could reflect an appreciable quantity of light. It seems exceedingly unlikely that under such circumstances the sky towards the meridian at night would remain to all appearance dark, while yet, according to Oudemann's view, the sky towards the Moon's place during total eclipse would be appreciably illuminated. It is undoubtedly true, as was pointed out by Professor Adams, that some substances (black cloth or velvet for instance) are rendered visible by rays falling obliquely, whereas rays falling square to their surface are absorbed. But certainly all the forms of non-luminous matter which we *know of* as tenanting the interplanetary spaces, would be better seen when placed as planets are when in opposition than as planets are when near inferior conjunction. I need not point out why this is, or that it is not so much a question of the reflective capacity of the particles themselves as of the proportion of their illuminated surface which is turned towards the Earth. Certainly a group of minute meteors near the Moon's place when she is full would send us much more light than the same group near the Moon's place when she is new.

But quite apart from this, there is an argument which appears to me to render Oudemann's reasoning altogether inadmissible. Assuming that the quantity of illuminated matter lying on *this* side of the Moon during total eclipse would *by itself* be appreciable, yet the much greater depth of the same matter lying beyond the Moon (and as well placed for the oblique illumination required) would be enormously greater, and would also be illuminated far more brightly. If a depth of 250,000 miles, full of such matter as Oudemann's theory requires, could give a certain quantity C of light, the ten millions of miles next beyond and towards the same direction would undoubtedly give more than the quantity 40 C of light, and the remaining eighty millions of miles lying yet further beyond, towards the neighbourhood of the Sun himself, would give a quantity of light which would even render this last-named quantity wholly inappreciable.

This is further illustrated by fig. 11, in which *em* represents the minute portion of faintly illuminated matter between the Earth and Moon, *ms′* the large portion of more highly illuminated matter lying beyond the Moon, towards, up to, and beyond the Sun's place.

It is to be remembered that Oudemann's theory, or rather that special part of it which relates to the coronal beams, was urged to explain the supposed mobility of the beams, as seen by Dr. Gould; and that all the other American observers in 1869 considered that the beams were stationary.

Fig. 11.—Illustrating Oudemann's Theory of the Corona.

And further, it should be noticed that a single observation of the fixity of coronal rays (as, for instance, Bruhn's observation in 1860) is more convincing than any number of observations of apparent motion. For, seen as the beams must needs be (if beyond our atmosphere) through a medium whose condition is probably very variable during a total eclipse, we can readily understand that their aspect should sometimes seem to vary, or even that they should appear and disappear under the observer's eyes. But the fixity of a coronal beam cannot so be explained away; and those who are familiar with the history of eclipse observations are aware that many of the positive observations of this sort are unexceptionable.

Now the great V-shaped gap in the corona of December 1870 (fig. 12; see also figs. 13 and 14, p. 211) was visible at widely separated stations; it is clearly recognisable in a photograph taken by the American observers in Spain; and it remained unchanged during the whole of totality.* A positive observation like this, relating to an object extending to a great distance (nearly half a degree) from the Moon's limb, seems altogether unmistakable in its import. When it is added that the bright inner corona was perceptibly depressed where this V-shaped gap existed, results of very great interest are suggested.

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* Since this was written I have had the opportunity of examining the best of Mr. Brothers's photographs (taken at Syracuse). This view, No. 5 of his series, far surpassing all other pictures of the corona in interest and value, shows the V-shaped gap opposite the south-eastern quadrant in an unmistakable manner. Indeed the gap is the most striking feature in the photograph. This disposes of the question.

Do not the observations made in December 1870 strongly support the view urged a year ago by many (myself among the number) that the corona is a solar aurora? *If the action of the radial solar forces which generate this aurora be supposed only to be more energetic over the spot-zone, and specially over the regions where spots actually exist, we should not only have an explanation of the so often noticed trapezoidal form of the corona, but also a suggested explanation of the observed association between the Sun-spot period and terrestrial auroras.*

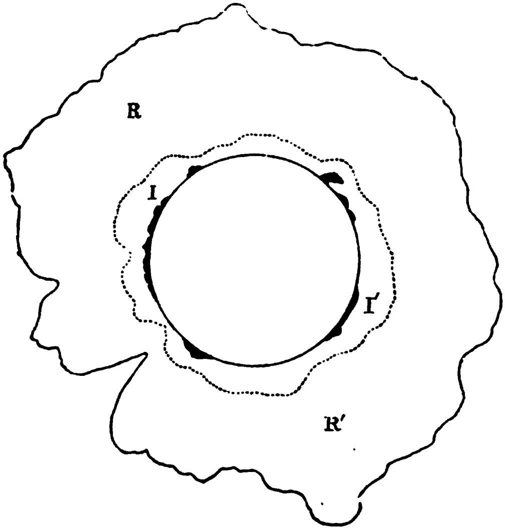

Fig. 12.—Lieut. Brown's drawing of the inner and outer Coronas (I I′ and R R′) during the Eclipse of December 1870.

Monthly Notices of the Royal Astronomical Society for January 1871.

NOTE ON THE CORONA.

A YEAR since, I had occasion to address the Astronomical Society on the subject of the corona. I then pointed out that no direct sunlight can illuminate the part of our atmosphere which lies towards and around the Moon's place; and the purpose with which I wrote was simply that the views I dealt with might be withdrawn from public attention. At that time it seemed to me, on the one hand, very unsafe to theorize about the actual constitution of the corona; but, on the other hand, it seemed demonstrable that the corona is a solar appendage. At present we are, I think, more favourably circumstanced; more especially on account of those photographic successes with which astronomers—as well workers as thinkers—have such good reason to be satisfied.

I propose to place before the April meeting of the Society certain views to which, as I think, the recent observations seem to point. At present, however, my object is merely to note what I take to be by far the most important contribution to our knowledge respecting the corona. I cannot regard the differentiation of the corona into two portions *

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* I would submit that the word 'defined' applied in the Report of the Council to the outline of the inner part of the corona, requires to be modified or explained; for undoubtedly the inner corona was not bounded by what is usually understood as a defined outline. The border of this part of the corona showed a rapid but not sudden degradation of brilliancy, having a perfectly soft outline, nowhere sharply defined. I venture, while there is yet time, to urge the adoption of convenient and expressive names for the phenomena presented during solar eclipses.

The word *photosphere* serves a very useful purpose, and I do not know that as an acquisition, simply because for more than a century and a half the distinction had been recognised by astronomers. Nor can I attach any signal importance to the proof afforded by recent observations that the corona has a great extension from the sun; nor to the confirmation of the long-disputed American observations; because both these points were in effect established before the eclipse took place.

The great result of the recent eclipse observations will be found, unless I mistake, to lie in the association now shown to exist between the configuration of the inner and brighter

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any other could be devised which would be nearly so suitable. But to the names *chromosphere* and *leucosphere* there are grave objections.

In the first place, the relation between the prominences and the layer of coloured matter at a lower level is such as to render the term *Sierra* employed by those who discovered the layer altogether more appropriate than such a word as Chromosphere. Secondly, the name Chromosphere implies that the coloured layer forms a spherical envelope, which the irregularity of its sufficiently well-defined outline shows not to be the case. Thirdly, the word is not properly formed, *Chromatosphere* being, I apprehend, the correct form.

The objections to the word *Leucosphere* are even greater. Such a word could not possibly be employed in descriptive astronomy without explanatory notes. And further it can scarcely be considered appropriate. For λευκός means white, and σφαῖρα means sphere; but the inner corona, when seen under favourable conditions, has not appeared white, and certainly it is not spherical. Furthermore, grave doubts exist whether the implied distinction between the inner and outer parts of the corona is more than apparent. It may be added that in all other combinations of the kind—as *atmosphere*, *photosphere*, and so on—(*hemisphere* belongs to another class) the first word of the compound is a substantive. There seems a valid objection to a change of plan in this respect.

But the great objection against both *Leucosphere* and *Chromosphere* consists in the utter unfitness of either for the purposes of descriptive writing. In this respect they differ wholly from the word *Photosphere*, which refers to a relation not likely to enter into descriptive passages; but objects such as those for which the names Chromosphere and Leucosphere have been suggested require expressive names. I can see no reason why the fine word *Sierra* should not be restored to its place in our books of astronomy; nor why, if it shall appear that a real distinction exists between the brighter and fainter parts of the corona, the former should not be called (as already by Schellen and Secchi) the *corona* and the latter the *glory*. Or else Professor Airy's mode of describing them might well be adopted, and one called the *ring-formed corona*, the other the *radiated corona*.

portion of the corona and its outer and more strikingly radiated portion. This is shown unmistakably by a comparison of the drawing of Lieut. Brown, fig. 12, p. 202, with the photographs of Mr. Willard and Mr. Brothers, figs. 13 and 14, p. 211. It was indeed strikingly evidenced during the eclipse of 1869, but not absolutely demonstrated. It seems to me a fact of the utmost importance and significance; more especially when combined with the seemingly established relations between the regions of greatest prominence-disturbance and the expansions of the inner part of the corona, and with that other relation which associates the spot-zones with the larger and more active prominences.

It might seem at first sight that the long radiations opposite the bright parts of the inner corona could be explained as due to the illumination of our own atmosphere in corresponding directions. But the simple consideration of the way in which our atmosphere is illuminated by the corona will show that no radiations extending outwards could make their appearance without corresponding expansion on either side, and a yet more marked extension inwards over the Moon's disk.* The uniformity of the light over the Moon's disk, its faintness, and especially its observed inferiority to that received 15′ from the Moon's limb, would suffice to disprove the imagined explanation, which is also, however, opposed to the simplest optical considerations.

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* It was with much pleasure that I heard Mr. Brothers read at the last meeting a letter from Dr. Balfour Stewart, in which views were expressed precisely similar to those above enunciated. In the same letter Dr. Stewart pointed out (as I had shown in the *Monthly Notices* for March 1870), that though our atmosphere towards the Moon's place is undoubtedly illuminated by light from the prominences, sierra, and inner corona, yet that the quantity of light received in this way can bear no higher proportion to the actual light of the corona and prominences than the atmospheric glare in full sunlight bears to such sunlight. It has been this argument—absolutely demonstrative, despite its extreme simplicity—which has caused me for many months past to feel complete certainty respecting the general nature of the corona.

I conceive we have now clear evidence of a form of action—but whether eruptive, electrical, or simply repulsive, is not as yet obvious—exerted outwards to enormous distances by the sun, and with maximum energy over the spot-zones, but local, variable, and probably intermittent.

Monthly Notices of the Royal Astronomical Society for March 1871.

ON THE SHALLOWNESS OF THE REAL SOLAR ATMOSPHERE.

In my treatise on the Sun I have pointed out at page 192 that the conspicuous nature of the darkening of the disc near the edge is a proof of the shallowness of the superincumbent atmosphere, and not, as is commonly stated, of that atmosphere's being enormously deep. And at p. 295 I indicate reasons for believing that the method by which the prominences and sierra have been studied when the Sun is not eclipsed, is not capable (save under highly exceptional conditions) of exhibiting the existence of the true solar atmosphere—that atmosphere, to wit, which causes the dark lines in the solar spectrum.

It will be known to all who read this communication that Professor Young of America, and Mr. Pye, independently recognised the existence of a highly complex atmosphere close by the solar photosphere. The slit of a spectroscope being placed tangentially to the limb, at the place where second contact was to occur, the spectroscopic field at the moment of totality and for several seconds after, was seen to be full of bright lines, 'every non-atmospheric line of the solar spectrum showing bright.'

The accuracy of this observation has been called in question. It is urged that the method of observing the uneclipsed Sun should be competent to show these bright lines if the supposed atmosphere have a real existence.

Now the competence of the last-mentioned method, so far as its power of obviating the effects of atmospheric illumination is concerned, cannot be questioned. For, indeed, as we know, Mr. Lockyer has, on one occasion, seen multitudes of the Fraunhofer lines reversed in this way. But because he has on all other occasions failed, while neither Zöllner, Respighi, nor Young has been favoured even with a single view of this sort, it is urged that no such atmosphere can exist, or that, at any rate, eclipse observers could have no better opportunity of recognising it with the spectroscope than those who have studied the uneclipsed sun. I would submit that this inference is erroneous, and that in one important respect observations made during eclipses have a great advantage over observations made when the sun is not eclipsed. It has been overlooked, as I opine, by those who urge the objection I am considering, that the image of the solar limb, whether as viewed in the telescope, or spectroscopically, is formed by the combination of diffraction-images of the several points of the real limb; and therefore (independently of irradiation, which, however, should also be taken into account) must needs extend beyond the true outline. We can tell, in fact, how great the extension is, since we know (experimentally) the dimensions of the diffraction-images of luminous points, for given apertures. A telescope which would not separate γ^2 Andromedæ, for instance, would *certainly* not be capable of showing (when armed with a suitable spectroscope) the bright lines of a solar atmosphere whose height subtended but about the fifth of a second—that is, of an atmosphere 80 or 90 miles in height; nor probably would it show these bright lines, even though the atmosphere were three or four times as high.

I think, therefore, that we are not justified in rejecting, or even in regarding as inconclusive, the observation made by Professor Young[*] and Mr. Pye. The inference would clearly be that the sierra cannot in any sense be regarded as the true solar atmosphere. Be it noted, also, that the evidence here considered is altogether independent of that on the strength of which I have been led to assert that in all probability the sierra is not of the nature of a solar envelope at all, but is made up of multitudes of relatively small prominences and of the remains of larger ones. So far as I know, the only circumstance on which the theory that the sierra is an atmospheric envelope was founded, was the supposed smoothness of its outline. This relates to the observations which led to the re-discovery of the sierra in 1868. But telescopic observations by Airy, Leverrier, Secchi, and many others in 1847, 1851, 1860, &c., had abundantly established the fact that its outline is commonly irregular. Respighi, in 1868, confirmed this with the spectroscope, and the fact is now generally admitted.

We have, I conceive, no escape from the conclusion that the prominences and sierra consist of glowing vapour which has been flung *through* the real solar atmosphere,—that atmosphere being highly complex and probably existing, especially near the photosphere, at an enormous pressure. What that medium is in which the prominences and sierra are seen remains to be shown.

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[*] This observation was completely confirmed during the eclipse of December 1871. It should be noticed that the credit of the observation must be assigned almost wholly to Prof. Young, since he alone used an analysing spectroscope. The integrating spectroscope used by Mr. Pye was adjusted by Prof. Young, who also instructed Mr. Pye in its use.

THEORETICAL CONSIDERATIONS RESPECTING THE CORONA.

IT may be questioned whether we are yet in a position to theorise safely respecting the corona; yet I feel that I need offer no apology for entering here upon the analysis of the evidence now available on the subject, with the object of determining towards what hypotheses that evidence appears to tend. Having lately had occasion, while preparing my treatise upon the Sun, to go through the principal records of former eclipses, I shall be able to avoid the mistake of giving *undue* weight to observations lately made;—that is, of giving them a value founded rather on their recentness than on their specific importance. It has always seemed to me specially necessary, if one would theorise safely, to attach proper weight to *all* the known facts; and I have sometimes been led to believe that the want of success with which, as a rule (save in a few highly exceptional cases) observers theorise on their special subjects, is to be looked for in the fact that their own observations acquire an exaggerated importance in their minds, the labours of others being unduly, though quite honestly, underrated. Precisely as the workers in some great edifice are not well placed to recognise its proportions as a whole, so it must commonly happen that the most skilful workers in science are precisely those who are least fitted to judge of the position to which—by others labours and their own—their special subject has attained.

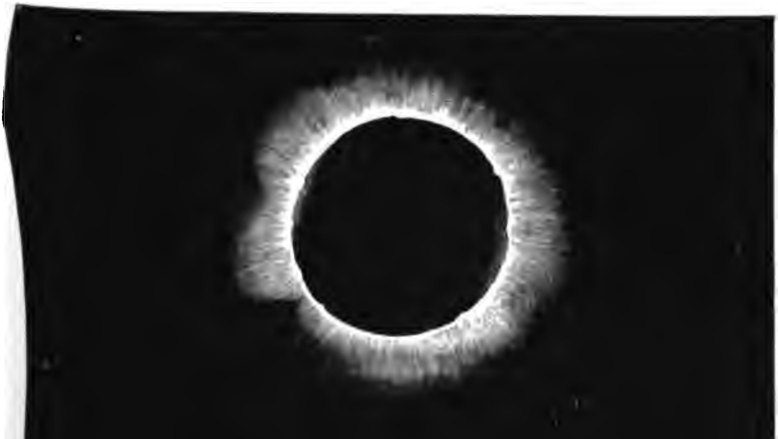

FIG. 13.

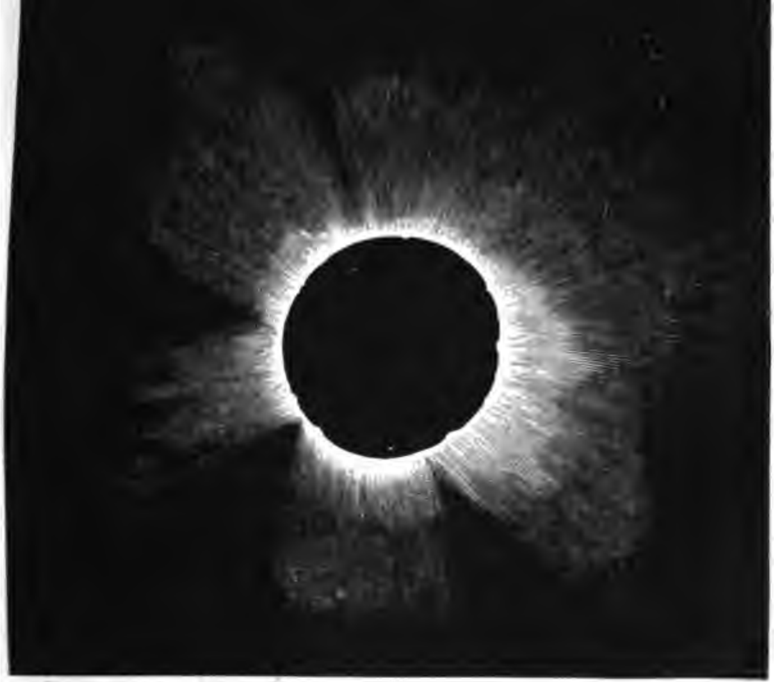

FIG. 14.

FIG. 13.—Mr. Willard's photograph, taken near Xerez.
FIG. 14.—Mr. Brothers's photograph, taken at Syracuse.

Certainly, there has been much to suggest such considerations in some recent enunciations of opinion respecting the corona. Many of the able spectroscopic workers who have dealt with the subject seem to regard spectroscopy as almost the only means of attacking the problem; experts in polariscopic analysis have at least not undervalued their special department of research; Mr. Brothers is convinced of 'the pre-eminent use of photography for determining points in dispute;' and some general observers appear to consider the two minutes' view they have had of coronal phenomena more likely to supply a correct answer to all our questions than either spectroscopy, polariscopy, or photography—to say nothing of the general observations made by others, or of the voluminous records of former eclipses.

If we consider what *new* information the recent eclipse has brought us, and combine that information with what had previously been ascertained, we shall probably have a better chance of arriving at satisfactory results than by limiting our attention to a few disjointed facts. Fortunately the attention of those, even, who are least familiar with the history of coronal research, need now no longer be distracted by any theories tending to explain the corona as a phenomenon of our own atmosphere. Men of science are now in (practically) unanimous agreement as to the solar nature of the corona; so that we can give our whole attention to the much more difficult, and much nobler problem, 'What is the real nature of this great solar appendage which total eclipses reveal to our view?'

I have long entertained the belief that the solar corona is due, in great part, to the existence of millions of meteoric systems having their perihelia for the most part much closer to the Sun than our Earth's orbit. The evidence recently obtained induces me to think that I have somewhat exaggerated the part which must be assigned to meteoric systems in accounting for coronal phenomena. Of course there can be no question about the existence of enormous numbers of meteoric systems in the position specified. It would be utterly unreasonable to suppose that the fifty-six meteoric systems recognised by Prof. Alexander Herschel, and those others (making up the number to more than a hundred) which Heis has recognised, bear any but a most minute proportion to the total number having perihelia within the Earth's orbit. The laws of probability will not permit such an assumption for a moment. Nor can it well be doubted that the density of perihelion-distribution increases towards the Sun's neighbourhood, in the case of meteors, precisely as in the case of comets; insomuch that portions of many meteoric systems would be much more intensely illuminated (surface for surface) than our Earth, or Venus, or Mercury; while in many cases myriads of meteors near the perihelia of their orbits must be rendered incandescent, if not absolutely vaporised, by the intensity of heat to which they are exposed. Even setting this probability out of the question, it remains—not a theoretical—but a demonstrated fact, that no inconsiderable quantity of light must come to us during total eclipse, from meteors lying towards the Sun's place and illuminated by his light.

But although we have thus undoubtedly found a *vera causa* for a portion of the coronal light, yet there are phenomena which seem to prove that another and larger portion remains unaccounted for. In a former paper I have pointed out the most marked peculiarity of the sort—the observed association, namely, between the expansions and depressions of the inner part of the corona, and those far-reaching radiations which form the principal feature of the outer corona. This is most strikingly shown in the negative of Mr. Brothers's best photograph, and is confirmed by Lieutenant Brown's drawing. But it is worthy of notice that during the eclipse of 1869, evidence was obtained which tended almost as strongly to establish the same association; and I am disappointed with myself, that, though I long and carefully examined the records (photographic and otherwise) of that eclipse, this important fact escaped my scrutiny. Now there is another relation, scarcely less significant when considered alone, but assuming a yet greater importance when combined with the last. Wherever the inner portion of the corona is depressed, there the coloured prominences are wanting and the sierra itself is shallow. Professor Roscoe, speaking of Mr. Seabroke's maps of the prominences (made before the eclipse), and Professor Watson's drawing of the inner corona, says:—'On comparing the two drawings thus independently made, a most interesting series of coincidences presented themselves. Wherever on the solar disc a large group of prominences was seen in Mr. Seabroke's map, there a corresponding bulging out of the corona was chronicled on Professor Watson's drawing; and at the positions where no prominences presented themselves, there the bright portions of the corona extended to the smallest distances from the Sun's limb. It of course follows, by combining the two relations, that the prominences are most numerous where the corona extends farthest, a fact noticed by Mr. Brothers, who tells us that throughout totality (as evidenced by his photographs), there was more coronal light on the west side of the Sun than elsewhere, and further, that 'the prominences were more numerous on the side where the corona was brightest.'* Indeed, on this side there was seen towards the close of the totality a perfect sickle of prominences,—according to the account given by Father Secchi.

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* The photograph of Lord Lindsay's series which was exhibited at the January meeting seemed to show the reverse. It appears, however, on a careful comparison of that photograph with Mr. Brothers's and the American one, that in some as yet unexplained way Lord Lindsay's has been inverted and reversed. The perfect agreement of the prominences when this correction is attended to leaves no doubt that such a mistake has been made.

Now, the point to be specially attended to here is the evidence of vertical disturbance (vertical with reference to the Sun's globe) extending to enormous distances from the photosphere. The notion that we have to do with objects of the nature of concentric atmospheric shells—advanced (as it seems to me) in the first place on very insufficient evidence—appears completely negatived. If the inner corona had presented no signs of association with the outer corona, we might have overlooked the very marked departure of the former's outline from concentricity with the Sun's, precisely as for several months the irregularity of the *sierra's* outline was overlooked or forgotten, and the atmospheric-shell character ascribed to it (by implication) in the adoption of the title *chrom(at)osphere.* But the observed association between the inner corona, and so obviously unshell-like an appendage as the outer radiated corona, leaves the shell theory, implied by the title *leucosphere*, altogether untenable. We might *fairly* claim from an atmospheric shell a respectable smoothness of outline, but we can authoritatively claim that it shall not associate itself, even in appearance, with a strikingly radiated and gapped appendage.

But equally the meteoric theory of the corona must be abandoned, at least so far as its claims to account for the special features of the corona (and therefore for the greater part of the coronal light) are concerned.

We seem to have unmistakable evidence of the action of vertical solar forces, or at any rate of forces directed outwards from the Sun's globe—though not necessarily exactly radial.

At the outset, the air of improbability which unquestionably surrounds this theory,[*] is to a certain extent removed by what we have learned respecting the formation of prominences. The evidence supplied by Zöllner and Respighi, to whose labours in this special department astronomy owes so much, suffices to show that prominences, as respects their first formation, are phenomena of eruption. For although Zöllner has divided the prominences into two classes—the cloud-prominences and the eruption prominences—yet he in no sense negatives the statement of Respighi, that the appearance of prominences is preceded by the formation of a rectilinear jet, either vertical or oblique, and very bright and well-defined. Respighi adds, that a jet of this sort, 'rising to a great height, is seen to bend back again, falling towards the Sun like the jets of our fountains, and presently the sinking matter is seen to assume the shape of gigantic trees more or less rich in branches and foliage.'

The velocity with which the gaseous matter of these prominences must pass the photosphere, in order to reach so great a height above it as Respighi has noticed in the case of some prominences, is as nearly as possible 200 miles per second, even if we neglect all the effects of resistance as the erupted gas rushes onward to the highest point of its excursion. Let this be specially noted. If the highest prominence yet seen by Respighi was the highest possible, and was wholly unforeshortened, we yet have proof of an eruptive action capable of sending out gaseous matter with the enormous velocity mentioned above; and this, be it remembered, is only the velocity with which the erupted matter crosses the level of the photosphere. Far beneath that level, at those depths, for instance, whence Zöllner assumes the prominence-matter to have been erupted, the velocity must have been far greater. If we also take into account the effect of the resistance which is undoubtedly encountered by the erupted gas during its flight, we have to add even more largely to the initial velocity. It is scarcely conceivable, all such considerations being duly weighed, that the initial velocity can be less than 300 miles per second; and it is far from inconceivable that the initial velocity may be two or three times as great.

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[*] It will be known to many of my readers that the theory is by no means a new one.

So far, be it remembered, we have been dealing with known facts; and the only way of avoiding the general conclusion above indicated, is by rejecting the supposition that prominences are phenomena of eruption. This will scarcely be regarded, however, as permissible, in the face of the reasoning of Zöllner and Respighi, and the evidence which they have adduced in its support. And besides we have independent means of knowing that *outside* the photosphere velocities occur which are comparable with those above referred to. I may cite, for instance, the motion of the bright points watched by Carrington and Hodgson in 1859, as well as the spectroscopic measurements of the velocity with which portions of the solar atmosphere are at times endowed.

Now, if velocities such as I have spoken of are produced by eruptive action exerted far beneath the photosphere, then there can be no question that any material erupted with the glowing hydrogen, and of considerably greater density, would retain, when passing the level of the photosphere, a much greater proportion of the velocity initially imparted. For example, instead of the velocity of 200 miles per second with which the matter of Respighi's great prominence *must* have crossed the photospheric level (to attain its observed height), any solid, liquid, or even dense gaseous matter flung out with the glowing hydrogen of that prominence, would probably retain a velocity of—say—240 miles per hour. In such a case it would reach a height exceeding that indicated by the greatest extension of the radiations observed in December 1870. A velocity (at the photospheric level) only one-half greater than this would, indeed, suffice to carry a body to a distance from the Sun equalling our Earth's. A velocity twice as great as that of the prominence-matter at the photospheric level, would carry a body for ever away from the Sun, never to have its velocity reduced to less than 125 miles* per second, even when it had passed away to stellar distances.

But certainly the theory that a large proportion of the coronal light is due to matter erupted from the Sun, is one which would require very strong evidence to render it acceptable. We have seen that the appearance presented by the inner and outer corona and the observed association between those regions and the sierra, lead directly to the theory that the corona is a phenomenon of eruption; and undoubtedly this theory is the one naturally suggested by the photographic views of the corona in this and the last eclipse, as well as by the drawings and descriptions of the corona as seen in preceding eclipses. It is impossible, for instance, to look either at the drawing made by Mr. Gilman of the eclipse of 1869, or at the very remarkable (and most strongly attested) picture of the corona as seen during the eclipse of 1868 at Mantawaloc-Kekee, without being impressed with the feeling that we have here pictures of eruptional phenomena. Still, all such natural and obvious conclusions are to be regarded by the true student of science with great distrust, and to be analysed with exceeding care, the natural senses being of all his faculties those which are most likely to lead the theoriser astray.

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* The velocity here spoken of is one of 400 miles per second; the least velocity required to carry a body altogether away from the Sun, is at the photospheric level, 380 miles (more exactly 379) per second. The velocity of a body which crossed the photospheric level at the former rate, would at an infinite, or practically infinite distance, be reduced to $\sqrt{(400)^2-(380)^2}$, or about 125 miles per second.

It will be well then to inquire whether the somewhat startling theory here dealt with is confirmed or disproved by such tests as we can at present apply to it.

The first and most obvious test is the examination of the photographic records, to see if any signs of the action of eruptive forces can be detected in the corona. Here I am disposed to lay great stress on the examination of the original negative of Mr. Brothers's best picture, Fig. 14, and it was with some interest that I availed myself of his kind permission to scrutinise it. The evidence it supplies is such as none of the positives supply, such as no artist perhaps could reproduce. It is such as to suggest in the strongest possible manner that the coronal radiations are phenomena of eruption. I was pleased to find, when I mentioned this view to Mr. Brothers, that Mr. Baxendell, of Manchester, had expressed precisely the same opinion of this remarkable record.* The appearances referred to are such as to show that some pictures of the corona as seen under exceptionally favourable conditions, are not, as has been commonly supposed, altogether idealized and in fact unwarranted, but merely represent with exaggerated distinctness features which have a real existence. This remark applies, for instance, to that remarkable drawing by Liais, of the eclipse of 1858, a copy of which appears at p. 326 of my treatise on the Sun, as also, though in a less degree (the exaggeration being in these instances less marked), to the picture by Feilitsch (p. 330), and that of the corona as seen at Mantawaloc-Kekee in 1868 (p. 334). As respects this last eclipse

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* I venture to express here my feeling that it is of the utmost importance that the evidence given by this photograph should be exhibited as fully as possible—in the report of the eclipse—even though it should appear that this could only be done at considerable cost. The extreme delicacy of details is such that nothing but the perfection of engraving can properly exhibit their nature. But I do not hesitate to say that if they *can* be reproduced they would be invaluable, in the present state of our knowledge respecting the corona.

I would invite special attention to the account given by Mr. Pope Hennessey, who observed at a station near Labuan. The luminous ring round the Moon 'was composed,' he says, 'of a multitude of rays quite irregular in length and in direction; from the upper and lower parts they extended in bands to a distance of more than twice the diameter of the Sun. Other bands appeared to fall towards one side; but there was no regularity, for bands near them fell away apparently towards the other side. When I called attention to this, Lieut. Ray said, "Yes, I see them; they are like horses' tails;" and they certainly resembled masses of luminous hair in complete disorder.' (The reader will be reminded here of the appearances resembling hanks of thread in disorder, seen during the eclipse of 1842.) The account is accompanied by a drawing precisely resembling such a drawing as I should make if I were to try to represent what I saw in Mr. Brothers's negative.

But now, leaving on one side the peculiar forms of the bands and rays, I would note that as to the existence of fine rays in the structure of the corona close by the Sun's limb, we have other evidence of a very striking nature. I refer to Mr. Gilman's picture in the report of the eclipse of 1869. In the letter from Sir John Herschel, read by Mr. Brothers at the March meeting of the Astronomical Society, there occurs this passage, referring to Mr. Brothers's photograph:—'I see nothing which gives me the idea of rays, or streaky radiation, such as appear in Gilman's picture, which, if it *could* be believed, would point to lunar mountains as the origin of the dark spaces, and bring the whole phenomenon within the distance of the lunar orbit.' Now we have seen that Mr. Brothers's negative *does* show precisely such rays, and furthermore Mr. Gilman's narrative is far too distinct, as is also that of Mr. Farrell, who observed with him, to leave any doubt as to the reality of the phenomenon. 'The corona,' says Mr. Gilman, 'was composed of an infinitude of fine violet, mauve-coloured, white, and yellowish-white rays, issuing from behind the Moon.' The corona 'looked,' says Mr. Farrell, 'as if it was the product of countless fine jets of steam issuing from behind a dark globe.' We cannot reject such testimony as this, coming from undoubtedly competent observers, and altogether inexplicable as due to mere illusion. Whatever conclusions this observation (as well as Mr. Pope Hennessey's, Lieut. Ray's, and the photographic negative) may seem to point to, must unquestionably be examined with attention. But the lunar explanation was not only negatived by the evidence obtained during the recent eclipse, but is completely disposed of by the considerations I adduced in the paper on Oudemann's Theory, p. 99. I have since had occasion to submit those considerations to Sir John Herschel, leaving him to judge of their weight, and he replied at once that, as I urged, any illumination derived from cosmical dust on *this* side of the Moon would be altogether *lost* in the illumination derived from the cosmical dust lying beyond, up to, and past the Sun's place. But it will be seen, at once, that the theory which regards the corona as a phenomenon of eruption, requires that these 'countless fine jets,' &c., should be seen, whenever the corona is viewed under exceptionally favourable conditions. The rush of matter cannot be conceived as taking place otherwise than in countless exceedingly fine jets, in jets corresponding perhaps in number to the countless minute prominences which produce the *sierra*. And in passing it must be noted that the actual quantity of erupted matter must be regarded as inconceivably minute by comparison with the Sun's mass, insomuch that the whole amount of matter present at any one moment in the corona (so far as this cause is concerned) might be outweighed by one of the least among the asteroids.

But it may now be well to remove certain difficulties which present themselves when the startling theory we are dealing with is carefully considered,—or rather it remains to be shown that the considerations on which such difficulties are founded tend in reality to afford striking evidence in favour of the theory.

In the first place, it will seem highly probable that some at least of the matter flung out from the Sun would have such velocities as I have referred to above as surpassing those required either to reach the Earth, or even to carry matter away altogether from the Sun's control. In this case, it would follow:—1st, that some of the erupted matter would from time to time salute our Earth; and, 2ndly, since other stars are suns like our own and may be presumed to behave in a similar manner—that matter erupted from the stars might cross the interstellar spaces and visit our own system.

Now regarding the erupted matter as, after cooling, meteoric in nature, it would undoubtedly follow from the first of these results, that more meteors should fall in the daytime than at night. For the night hemisphere of the Earth would be saluted only by meteors not belonging to these solar eruptions. In the daytime solar meteors (if one may so term them) would be added, since meteors arriving from the Sun must needs fall on the hemisphere turned towards him at the moment of the arrival; there would be a preponderance, then, in favour of day-falls. Now the only class of meteors we can make comparison by, is the class of aerolites, since the fall of these only can be recognised in the day-time. And it is a fact, according to the testimony of Humboldt, Heis, and others, that these aerolites fall, on the whole, somewhat oftener in the day time than at night.

But, if some meteors ejected in this way from *stars* should reach our system, we should expect that they would exhibit some signs of having been expelled with a velocity exceeding that barely necessary to carry them away from their parent sun. In other words, we should expect that *some* meteors would exhibit velocities exceeding those which the Sun can impart to masses drawn by him from outer space. Now it has been a source of grave perplexity to all who have studied the details of meteoric astronomy that some meteors *do* actually traverse our atmosphere with a velocity exceeding by many miles per second that which they could possibly have, even though after having been drawn by the Sun from an infinite distance, they encountered the Earth full tilt at the moment when she was in perihelion. This velocity cannot exceed 45 miles[*] per second, whereas we have satisfactory

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[*] In such an inquiry the effect due to the Earth's rotation, as also that due to her own attraction upon the meteors, may safely be neglected by comparison with the much greater velocities of the meteor and of the Earth in perihelion, which we add to obtain the meteors' apparent or relative velocity. But it might seem as though, in some cases, the outer planets, and especially Jupiter, might impart an additional velocity to meteors passing near them on their way to the Earth, and that thus the observed excess might be accounted for. Now, passing over the fact that only a few meteors have arrived on such paths as would at all correspond with the imagined explanation, the following considerations will suffice to show that the effect which even Jupiter would have in increasing the velocity of a meteor is not sufficient to account for the observed velocities. (It will be obvious that Jupiter and Saturn could not both act to accelerate a meteor in the imagined way, unless in cases so exceptional that not one instance could occur out of countless millions of observed instances):—

Conceive that while a meteor approaches the Sun from an infinite distance (*i.e.* a distance practically infinite) Jupiter is at rest, in the line of the meteor's course, at a distance J from the Sun; and that at the moment the meteor is about to impinge on the surface of Jupiter, the planet is suddenly removed altogether away. Then in this imaginary case, it is obvious that Jupiter would produce a much greater effect than he can by any possibility produce in increasing the velocities of meteoric bodies. Now it is easy to determine what would be the velocity of a meteor, subject to the imagined influence, when it subsequently crossed the Earth's orbit. The equation of motion before the meteor reached Jupiter's place would be

$$\frac{d^2 x}{d t^2} = -\frac{\mu_S}{x^2} - \frac{\mu_J}{(x-J)^2},$$

where μ_S is the attractive force of the Sun's mass at a unit of distance, μ_J that of Jupiter's. We get, then,

$$\left(\frac{d x}{d t}\right)^2 = C + \frac{2\mu_S}{x} + \frac{2\mu_J}{(x-J)};$$

evidence of meteoric velocities of 70, and even up to 80 miles per second.

C = 0, and putting j for the radius of Jupiter, the velocity (v) of the meteor just as it is about to reach the surface of Jupiter is given by the equation,

$$\left(\frac{dx}{dt}\right)^2 = v^2 = \frac{2\mu_s}{J+j} + \frac{2\mu_J}{j}.$$

The equation of the subsequent motion is

$$\frac{d^2x}{dt^2} = -\frac{\mu_s}{x^2},$$

giving

$$\left(\frac{dx}{dt}\right)^2 = v^2 = C' + \frac{2\mu_s}{x}. \qquad \text{(i)}$$

When

$$x = J + j, \quad v^2 = \frac{2\mu_s}{J+j} + \frac{2\mu_J}{j},$$

so that

$$\frac{2\mu_s}{J+j} + \frac{2\mu_J}{j} = C' + \frac{2\mu_s}{J+j},$$

and therefore

$$C' = \frac{2\mu_J}{j};$$

hence (i) becomes

$$v^2 = \frac{2\mu_s}{x} + \frac{2\mu_J}{j}. \qquad \text{(ii)}$$

Now in the case of a meteor crossing the Earth's orbit, after being brought by solar influence alone from infinity, the velocity would be given by the equation

$$v^2 = \frac{2\mu}{x},$$

when for x was substituted the radius of the Earth's orbit. We know that this velocity is about 25·7 miles per second. And again, it is easy to calculate the value of $\frac{2\mu_J}{j}$, which is, in fact, the expression for the square of the velocity with which a body approaching Jupiter under his sole influence from infinity would reach his surface. This is easily shown to be rather less than forty miles per second. Hence (ii) becomes

$$v^2 = (25{\cdot}7)^2 + (40)^2$$

whence v is about 47½ miles per second. This is a considerable increase, but when combined with the Earth's perihelion velocity of 18½ miles per second, it amounts to but 66 miles per second, and therefore still falls considerably short of authenticated meteoric velocities. But it need hardly be said that the actual influence of Jupiter can never approach in value that above estimated on an imaginary hypothesis.

It is worthy of notice, and has an important bearing on meteoric astronomy, that the possible influence of Jupiter in increasing the velocity of bodies which

When we remember also the observed association between certain meteoric systems and comets, similar evidence will be recognised in the hyperbolic figure of some cometic orbits, since no comet approaching the Sun can possibly be caused to travel in a hyperbolic orbit by his attractive influence alone. The hyperbolic figure is proof positive that comets whose orbits exhibit that figure have entered the domain of our Sun with considerable velocities imparted to them in some as yet unascertained way. As such a comet necessarily passes away from the domain of some other star with such velocity, it follows that neither has such other star by its attractive powers generated the whole of the comet's velocity. And as there is no limit to the application of such considerations, there seems no other way of explaining the interstellar velocities of the comets which have hyperbolic orbits than by tracing back their course to the moment when their substance was ejected from some star with a velocity exceeding, by many miles per second, that with which a body would reach that star if attracted from an infinite distance by the star's sole influence. Granted that hyperbolic orbits exist, it is unquestionable that they are not *due* to the stellar attractions, however perfectly the

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have approached the Sun's surface from enormous distances is very much less; for equation (ii) becomes in this case

$$v^2 = (379)^2 + (40)^2$$

whence v is about 382, corresponding to an increase of but three miles per second.

[The following formula is convenient for comparing the maximum velocity with which a body moving from infinity would reach the surface of either of two globes under their sole influence respectively. Let the radius of one be R, and its mean specific gravity C_R; the radius of the other r and its mean specific gravity C_r; the respective maximum velocities being V and v. Then $\frac{V^2}{v^2} = \frac{R^2 C_R}{r^2 C_r}$. The same relation holds if V and v be the velocities acquired in falling from heights H and h respectively, where $\frac{H}{h} = \frac{R}{r}$.]

motion of a comet in such an orbit corresponds, as we know, with the theory of gravitation.*

It would follow, if meteors or some meteors were star-expelled bodies, that their constitution, when examined microscopically or under chemical analysis, would exhibit some traces of their origin. In Part II. of this paper I propose to consider the very striking evidence we have on this point. I shall touch also on some other evidence in favour of the theory here dealt with—a theory which, startling as it appears, seems yet to accord, better than any other, with what is at present known respecting the Corona.

Monthly Notices of the Royal Astronomical Society for April 1871.

II.

BEFORE entering upon the consideration of the remaining portion of my subject, I propose briefly to discuss some questions which have been raised since the former part was written. I refer in particular to the meteoric theory of the Corona—that is, the theory that a large proportion of the light of the Corona is due to the existence of countless hosts of meteors in the Sun's neighbourhood. I have mentioned already my belief that I had somewhat exaggerated the share which must be assigned to meteoric systems in accounting for coronal phenomena.† But this admission must not be understood as having any reference whatever to opinions which I have publicly expressed. On the contrary, all my *statements* respecting the competence of the meteoric theory to account for the phenomena of the Corona, have been so guarded that I find nothing in them to modify. In speaking of a change of opinion, I have referred only to that view of the subject which I had been disposed to entertain in my own mind, not to any opinions which I had definitely enunciated. But I am not concerned at present to indicate how far, or how little, my own opinions have changed. Objections have recently been raised which would tend, if admitted, to invalidate the meteoric theory altogether as a means of explaining any portion of the coronal phenomena.

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* We may suppose, indeed, that in some few instances (*i.e.* relatively few), planets like Jupiter and Saturn may have given to parabolic cometic orbits a hyperbolic figure; but it seems scarcely admissible to suppose that this is otherwise than exceptional.

† In a paper by Prof. C. A. Young, of America, entitled 'Note on the Spectrum of the Corona,' (one of a series of highly suggestive and valuable contributions to the theory of the subject), he says, 'Although I am not able to admit with Mr. Proctor that the whole explanation of the Corona is involved in the presence of such meteoric particles, yet it cannot be doubted that they are very numerous; and any that may come within 250,000 miles of the solar

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surface must become incandescent,' &c. I cannot remember having on any occasion asserted that the *whole* explanation of the Corona is involved in the existence of meteoric systems near the Sun. I could quote many passages implying that I hold the contrary opinion. I may have written at times about the meteoric explanation as founded on a *vera causa* without discussing other *veræ causæ*; but it is not, therefore, to be inferred that I have doubted the reality of these others. I would notice in particular that I have always believed in the atmospheric nature of a portion of the light seen around the Sun during totality—even at the middle of the totality. In the paper on the Solar Corona and Zodiacal Light, I note that the illumination of our atmosphere by the light of the prominences and sierra should result in 'a faint diffused light diminishing towards the neighbourhood of the Moon,' and 'extending over the Moon's disc (since it would illuminate the air between the observer and the Moon's body).' I need hardly observe that the same obvious reasoning which showed me that the prominences and sierra must produce this kind of illumination, convinced me also that the real solar Corona (extending beyond the highest prominences) must be added to the causes of this atmospheric illumination; but in a paper written expressly to show that there *is* such a solar Corona, I was not free to assume in the opening paragraphs the very point I sought to prove. As surely as the visibility of the prominences and sierra implies the existence of an atmospheric halo due to their light, so surely the visibility of a real solar Corona implies the visibility of an atmospheric halo due to the light of that Corona. That both haloes are very faint compared with the real solar Corona follows from the reasoning given in the above-mentioned paper (pp. 177-181), that is (as Prof. Young and Dr. Balfour Stewart have since severally shown) from the darkness of the Moon's disc during totality, and from the faintness of the ordinary glare round the Sun by comparison with the light of the solar disc.

These objections must be dealt with here, because they touch the very basis of the theory I am now considering. I refer to those remarks in Sir W. Thomson's able address at the meeting of the British Association in Edinburgh, in which he referred to the opinions he had once entertained respecting the meteoric origin of the solar heat.

The meteoric theory of the solar heat-supply cannot be regarded as demonstrated, or even (considered as the sole source of the solar heat) as demonstrable; nor am I here anxious to support it in any way. But it is important to notice that the meteoric theory advanced and eventually abandoned by Sir W. Thomson is not the theory to which recent astronomical discoveries have pointed; nor can the reasoning which Sir W. Thomson has advanced as demonstrative against his own theory be urged with equal force (if with any force at all) against those views as to meteoric matter in the Sun's neighbourhood, which could alone be now advocated by the student of meteoric astronomy. It must be remembered that Sir W. Thomson's theory related to meteors circling chiefly within the orbit of the earth, and he was led to abandon it 'because Leverrier's researches on the motion of the planet Mercury, though giving evidence of a sensible influence attributable to matter circulating as a great number of small planets within Mercury's orbit, showed that the amount of matter which could possibly be assumed to circulate at any considerable distance from the Sun must be very small;' therefore, 'if the meteoric influx taking place at present is enough to produce any appreciable portion of the heat radiated away, it must be supposed to be from matter circulating round the Sun within very short distances of his surface. The density of this meteoric cloud would have to be supposed so great that comets could scarcely have escaped, as comets actually have escaped, showing no discoverable effects of resistance, after passing his surface within a distance equal to one-eighth * of his radius.' But recent discoveries respecting meteors point to a totally different solution of the difficulty here considered. We are neither bound to show that the greater part of the meteoric matter available for the purpose in question is at any time present within the Earth's orbit, nor that the matter which is at any time so placed (or rather at a less distance from the Sun than our Earth) is the same matter which is similarly circumstanced at another time. All that we know respecting meteor-systems teaches us to regard them as, for the most part, travelling in very eccentric orbits, only a small part of each orbit lying at a less distance than the Earth from the Sun, and therefore only a small portion of each system being at any time nearer to the Sun than the Earth is. And further, we have every reason for believing that only a very small proportion of the meteoric systems travel near to the plane of the Earth's orbit. Among the meteoric orbits there is every variety of inclination; and therefore we have not to deal with matter which is collected in or near the plane of the ecliptic, but with matter completely enveloping the Sun on all sides. It is not a disc or very flat spheroid, but a sphere of meteoric matter, that we are concerned with in considering those portions of such matter which lie nearer to the Sun than our Earth does. It needs but a slight acquaintance with the laws of planetary motion to see that neither the motion of the Earth (and the inappreciable change in the length of the year), nor the motion of Mercury (and the slowness of the change in the position of his perihelion), can afford such significant evidence respecting the quantity of meteoric matter existing within given distances of the Sun, as was formerly supposed.

In like manner, another objection which Sir W. Thomson

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* It should be, I conceive, 'one-*fifth*,' the comet referred to being that of the year 1843.

has urged against his former views loses much of its force when considered with reference to accepted meteoric theories. 'Spectrum analysis,' says Sir W. Thomson, 'gives proof finally conclusive against the hypothesis that the Sun's heat is supplied dynamically from year to year by the influx of meteors. Each meteor circulating round the Sun must fall in along a very gradual spiral path, and before reaching the Sun must have been for a long time exposed to an enormous heating effect from his radiation when very near, and must thus have been driven into vapour before actually falling upon the Sun. Thus, if Mayer's hypothesis is correct, friction between vortices of meteoric vapours and the Sun's atmosphere must be the immediate cause of solar heat; and the velocity with which these vapours circulate round the equatorial parts of the Sun must amount to 435 kilomètres per second. The spectrum test of velocity applied by Lockyer showed but a twentieth part of this amount as the greatest observed relative velocity between different vapours in the Sun's atmosphere.'

Now if this objection is sound as against the meteoric theory of the solar-heat supply, it is sound also as against the very existence of meteoric systems close to the Sun, a conclusion which very few will be disposed to admit after what has recently been discovered respecting meteors and comets. But I apprehend that the objection is obviated by precisely the same reasoning which is valid against the former objection. Undoubtedly if meteoric matter came in vortically around the equatorial parts of the Sun and in the direction of planetary motion, we might possibly expect to find some spectroscopic evidence of the existence of their vaporous substance, moving as it would with a velocity exceeding more than 200 times that due to the Sun's rotation. Even in this case it would appear venturesome to assert that the want of such evidence was 'proof finally conclusive against' the existence of the meteoric vortices. For the meteoric matter being brought to rest (relatively to the Sun) by friction, there must be all possible rates of motion between 240 miles per second and rest with respect to the Sun's globe. The bright lines due to the vaporous meteors would therefore be widened so as to cover the space between their normal position and the positions due to the maximum velocity of 240 miles per second. That thus widened, and proportionately faint, they would be discernible as bright lines on the bright background of the solar spectrum, may be gravely doubted; that they *must* be so discernible may be safely denied. But the actual circumstances are very different even from those here considered. All the evidence recently obtained respecting meteors tends to show that those which approach the Sun neither 'travel around his equatorial parts,' nor move on a direct course, nor with a velocity that can be definitely assigned. Our Earth encounters more than 100 meteor-systems (according to the observations of Heis, Alexander Herschel, and others); and though none of these systems probably pass near the Sun, yet we can infer from them what must be the characteristics of the millions on millions of meteoric systems which (according to all reasonable probability) belong to the solar system. We must conclude, then, that the systems which pass near the Sun are inclined in all possible directions to the solar equator, have every possible degree of eccentricity (and, therefore, every degree of velocity between 240 and 379 miles per second), and travel both in direct and retrograde courses. That the spectroscope would afford any evidence of the existence of these multifarious forms and degrees of motion, existing in systems which probably include every variety of elementary constitution, and further modified by the effects of frictional resistance, so that every velocity down to relative rest must be included among the meteoric movements, is utterly improbable, to say the least.

It will be understood that if the objections here considered were valid, they would affect the theory that the Sun expels meteoric matter from his interior, as fatally as the general theory that meteoric systems are circulating in countless myriads around the Sun's globe. For the Sun is but one among the unnumbered millions of suns which exist throughout space; and if our Sun expels meteoric matter, it must be inferred that the stars act in like manner. And objections against the existence of meteoric systems around the Sun—which cannot possibly be sun-expelled—would be objections against star-expelled meteors, and so inferentially against the expulsion of meteoric matter from the Sun. It is on this account that I have thought it necessary to consider the objections dealt with above.

If any of the meteors which reach our Earth have really been expelled either from our own Sun or from his fellow-suns, we might expect that their structure, as well microscopic as chemical, would exhibit some signs of the circumstances under which these bodies had their origin.

Now as respects the microscopic structure of meteors, although we have much interesting information, and though some facts are known which seem scarcely explicable save on the strange theory I am considering, yet it must be admitted that there is much that is perplexing. Since I wrote the first part of this paper, I have had the opportunity of inspecting a large number of Mr. Sorby's singularly beautiful specimens, with his own instruments, and with the advantage of his own unrivalled experience to explain those facts which otherwise would have had little meaning for me. He also kindly gave me copies of all his papers on the subject, and these I have carefully studied. Space will not permit me to discuss here the various facts on which Mr. Sorby's reasoning and his (hypothetical) conclusions have been based. It must suffice for me to state that, while there remain many sources of perplexity in every part of the subject, he still considers the general conclusion which he published in 1864 as the most probable, and that, in fact, no other seems available. How far this conclusion is in accordance with the theory we are upon, the reader shall judge. 'The most remote condition of which we have positive evidence,' he wrote in 1864, 'was that of small detached melted globules,* the formation of which cannot be explained in a satisfactory manner, except by supposing that their constituents were originally in the state of vapour as they exist in the Sun.' He found evidence that the meteors had been in the state of vapour while under enormous pressure, and 'in mountain masses.' It certainly seems difficult to understand where and how the substance of meteors could have been in this state, save within an orb as intensely heated and as vast as our Sun and his fellow-suns.

The evidence from the chemical structure of meteors is even more striking.

If we consider the circumstances under which the meteors are supposed (according to this theory) to be expelled from the Sun or stars, and remark the evidence we have respecting the existence of hydrogen in other suns than ours, we shall see the probability that some among the meteors which reach us would show signs of having been once surrounded by intensely hot hydrogen, existing at an inconceivably vast pressure. For iron, which is so frequently present in meteoric masses, if solidified under such conditions, would condense within its substance a considerable proportion of hydrogen. Now, we have evidence on excellent authority that meteoric iron contains a larger amount of occluded hydrogen than malleable iron can be impregnated with. The late Professor Graham examined a piece of the Lenarto meteor, constituted, according to Werle's analysis, of 90·883 parts of iron, 8·450 parts of nickel, 0·665 of cobalt, and 0·002 of copper. When a volume of 5·78 cubic centimètres of this iron was heated to redness, 'gas came off rather freely; namely, in 35 minutes 5·38 cubic centimètres, in the next 100 minutes 9·52, and in the next 20 minutes 1·63 cubic centimètres;' in all, in rather more than 2½ hours, no less than 16·53 centimètres, or about three times the volume of the iron itself. 'The first portion of the gas collected had a slight odour,' says Professor Graham;* 'but much less than the natural gases occluded by ordinary iron. It did not contain a trace of carbonic acid.' The second portion of the gas collected (consisting of 9·52 cubic centimètres) gave of hydrogen 85·68 parts per cent., the rest consisting of nitrogen and carbonic oxide. 'The Lenarto iron appears, therefore, to yield 2·85 times its volume of gas,' says Professor Graham, 'of which 86 per cent. nearly is hydrogen, the proportion of carbonic oxide being so low as 4½ per cent.' But 'the gas occluded by iron from a carbonaceous fire is very different, the prevailing gas then being carbonic oxide. For comparison, a quantity of clean horseshoe nails was submitted to a similar distillation.' This iron gave 2·66 times its volume of gas; the first portion collected contained only 35 per cent. of hydrogen, 50·3 per cent. being carbonic oxide, 7·7 per cent. carbonic acid, and 7 per cent. nitrogen; the second portion gave no carbonic acid, but 58 per cent. of carbonic oxide, and only ·21 per cent. of hydrogen.

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* It may be added that Prof. Stokes considers the polariscopic observations of the Corona to indicate the presence of minute crystals of metal, travelling radially from the Sun. Mr. Ranyard has shown also that the polariscopic observations of the zodiacal light imply not only that the light is due to the reflection of solar light, but that the matter reflecting such light either exists in particles so small that their diameters are comparable with the wave-lengths of light, or else that such matter is capable of giving specular reflection.

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* Not having Graham's original paper by me, I quote these passages from extracts in Mr. Mattieu Williams' *Fuel of the Sun*, where the theory of the expulsion of meteors from the Sun is enunciated and supported—on grounds, however, not always strictly in accordance with dynamical principles.

On these results Professor Graham reasons as follows:—'It has been found difficult to impregnate malleable iron with more than an equal volume of hydrogen under the pressure of our atmosphere. Now, the meteoric iron (this Lenarto iron is remarkably pure and malleable) gave up about three times that amount without being fully exhausted. The inference is that *the meteorite had been extruded from a dense atmosphere of hydrogen gas*, for which we must look beyond the light cometary matter floating about within the limits of our solar system. . . . Hydrogen has been recognised in the spectrum analysis of the light of the fixed stars by Messrs. Huggins and Miller. The same gas constitutes, according to the wide researches of Father Secchi, the principal element of a numerous class of stars, of which *a Lyræ* is the type. *The iron of Lenarto has no doubt come from such an atmosphere, in which hydrogen greatly prevailed. This meteorite may be looked upon as holding imprisoned within it, and bearing to us, the hydrogen of the stars.*'

Other circumstances relating to the Corona itself seem to require some such theory as that we are dealing with for their elucidation.

The coronal spectrum, although not by any means identical with the spectrum of the terrestrial aurora, shows yet such a resemblance to this spectrum as to indicate that the corona is in part due to a perpetual solar aurora. Such at least is the theory to which many profound reasoners have been led by the study of the coronal spectrum. But a difficulty had existed in determining how electrical action could be excited where we see the light of the Corona.*

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* When Prof. Reynolds exhibited (at the last meeting of the British Association) the very beautiful electrical Corona by which he illustrates the auroral

The theory we have been dealing with would remove this difficulty, for the rush of the erupted matter, even through the rare medium existing round the Sun, would produce precisely the effect which the coronal theory requires. The fact that one of the lines of the coronal spectrum belongs to the spectrum of iron may be regarded as supplying subsidiary evidence of some weight.

I have already referred to the fact that under close telescopic scrutiny the Corona presents close by the Sun an appearance as though countless thousands of jets were issuing from the photosphere. But it may be asked whether any direct evidence of an outrush of matter has ever been obtained. It might well happen that no such evidence was available; for, as I have mentioned, the actual volume of the erupted matter must be supposed to bear but the minutest possible proportion to the volume of the prominences. The swift motion of the erupted matter would not tend, perhaps, to add to the difficulty of detection, because the effects of that motion at the Sun's distance would scarcely be appreciable, even in powerful telescopes. But it would be difficult to distinguish the erupted matter by its appearance, and as its light would give a continuous spectrum (owing to the enormous compression of the issuing jets), it would be wholly impossible to detect the existence of this matter by spectroscopic analysis. It may be ques-

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theory of the Corona, Prof. Tait remarked that this theory had been rejected by men of science. It is difficult to understand on what grounds this remark was founded. I cannot find that any man of science has expressed an opinion adverse to the auroral theory. Dr. Balfour Stewart, General Sabine, and others, have used arguments respecting the prominences (before the nature of these was known) which may now be fairly applied to the Corona, while Prof. Young of America, Prof. Reynolds and others in England, and several Continental physicists, have spoken favourably of the auroral theory as directly applicable to the phenomena of the Corona. The noteworthy point is not, however, that there is such good authority in favour of the theory, but that not one man of science has definitely expressed an opinion adverse to it.

tioned whether the brilliant flakes seen by Mr. Gilman in the large prominences visible during the eclipse of 1869 can be regarded as in any way related to the subject we are upon. These flakes 'stood out,' he says, 'as if totally unconnected from the rest of the prominence.' But their size, as described and pictured by him, forbids us to believe that they could have been masses of erupted matter; though it is by no means impossible that they may have been clusters of many such masses resulting from volleyed discharges.

A phenomenon observed by Dr. Zöllner seems less questionably related to our subject. Observing the Sun on June 27, 1869, he noticed that as soon as he brought the slit of the spectroscope close to a certain part of the Sun's limb, *where the prominences were particularly long and bright*, brilliant linear flashes passed through the whole length of the dull spectrum, over the limb of the Sun, about three or four minutes' distance from the latter. 'These flashes,' he says, 'passed over the whole of the spectrum in the field of view, and became so intense at a certain point of the Sun's limb as to produce the impression of a series of electrical discharges rapidly succeeding one another, and passing through the whole spectrum in straight lines. Mr. Vogel, who afterwards, for a short time, took part in these observations, found the same phenomenon at a different portion of the Sun's limb, *where protuberances also appeared*.' Zöllner remarks that 'the phenomenon can be explained by the hypothesis that small intensely incandescent bodies moving near the surface of the Sun emit rays of all degrees of refrangibility, and produce flashes of a thread-like spectrum as their image passes before the slit of the spectroscope.'

To these considerations may be added some which are connected with the aspect of the solar photosphere. For instance, the researches of De la Rue, Stewart, and Loewy seem to prove that 'the faculæ of a spot have been uplifted from the very area occupied by the spot, and have fallen behind from being thrown up into a region where the velocity of rotation is greater.' This, of course, would correspond with what the theory we are considering would suggest. 'And it may be noticed' (here I quote from a paper of my own in 'Fraser's Magazine,' for April 1871) 'that, regarding spots as phenomena of eruption, that is, as *beginning* with eruption, we can find a reason for their occurrence being associated, as Mr. De la Rue and his colleagues believe, with the relative proximity of the planets. For eruptions and earthquakes on our own Earth, stable as its substance undoubtedly is by comparison with the Sun's, have been observed to occur more frequently when the Moon is in perigee; and Sir John Herschel has explained the predominance of active volcano and earthquake regions along shore-lines as depending on the seemingly insignificant changes due to tidal action. How much more, therefore, might we expect that the solar equilibrium would be disturbed by planetary action, when all that has been revealed respecting the Sun tends to show that the mightiest conceivable forces are always at work beneath his photosphere, one or other needing only (it may well be) the minutest assistance from without to gain a temporary mastery over its rivals. And if, as recent observations tend to show, the mightiest of the planets sympathises with solar action; if when the Sun is most disturbed the belts of Jupiter are also subject (as of late and in 1860) to strange phenomena of change; how readily do we find an explanation of what would otherwise seem so mysterious, when we remember that, as Jupiter disturbs the mighty mass of the Sun, so the Sun would reciprocally disturb the mass of the largest of his attendant orbs.'*

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* Since these papers were written, observations made by Father Secchi, of Rome, and Prof. Young, of America, have gone far to demonstrate the theory

here dealt with. Father Secchi has found that the eruptive prominences are confined to the solar-spot zone, and also that in their substance there are several metallic elements. Prof. Young has witnessed an eruption in which hydrogen wisps were carried from a height of 100,000 miles to a height of more than 200,000 miles in ten minutes; the calculated time for a projectile *in vacuo* traversing this distance and brought to rest at a height of 200,000 miles being 25′ 56″. Hence the retardation of the hydrogen must be enormous. A full account of Prof. Young's observations will be found in the second edition of *The Sun.*

THE SUN'S JOURNEY THROUGH SPACE.

Few of the discoveries made by astronomers are more surprising than that of the Sun's motion through the celestial spaces. Followed by his train of attendants—planets, asteroids, comets, and meteoric systems—he is ever rushing onwards through space with a velocity of which the human imagination is unable to form any adequate conception. Whether the vast orbit which he must in reality be pursuing is of any regular figure, or rather consists of myriads of interlacing loops; whether in the former case there is some vast central orb around which his motions are directed, or whether the orbit is simply regulated by the gravity of the scheme of fixed stars, without any preponderant mass at the centre; whether, in fine, the Sun is an attendant star, or is himself one of the regulating orbs of the sidereal scheme—on all these points astronomy is as yet silent. Speculation has, indeed, suggested many interesting surmises, grounded on more or less probable evidence; but as yet no theory founded on an exact examination of the results of systematic observation has been presented to the world. Nor is it likely that astronomers will quickly be able to systematise the motions of the stars. The Copernicus of the sidereal system is not to be expected for many generations, perhaps not for thousands of years. Nay, if it befitted us to doubt after the achievements of our Newtons and our Herschels, we might fear that the great problem of co-ordinating the motions of the fixed stars into a single scheme is one which it will never be given to the human race to triumph over.

What has already been done, however, is well worthy of careful study. In fact, it may be doubted whether the full complexity of the problem which has been solved by our astronomers has been thoroughly appreciated. I wish to exhibit the nature of the results which have been obtained, and then to discuss some peculiarities which, without in any sense throwing doubt upon the justice of the conclusions to which astronomers have arrived, yet serve (unless I mistake) to prove in the clearest manner that the assumptions on which the problem of the Sun's motion has been solved require modification. The work of our astronomers resembles, in a sense, the famous work of Adams and Leverrier, when they spread forth the subtle webs of their analyses to capture the unseen planet whose influence had so long been felt upon the outskirts of the solar system. Just as their assumptions respecting the mass and distance of the great unseen were incorrect, yet led to a correct result, so the assumptions on which our astronomers have founded their determination of the Sun's motion in space may be now shown, by means of that very determination, to have been wholly incorrect.

Let us first rightly grasp the nature of the problem which the elder Herschel set himself.

He argued that if our Sun is moving through space, the effects of his motion must generally be on this wise:—The stars in those regions of space towards which the Sun is moving must seem to 'open out,' precisely as the trees of a forest seem to 'open out' as we approach them. The stars in the opposite region must seem to close in to a corresponding extent. But the most marked effect must appear in the stars which are on or near the celestial circle midway between these two regions. All such stars must be affected by a 'backward drift,' corresponding precisely in degree to the rate of the Sun's advance.

It will be observed that hitherto the question of the stars' distance has not been introduced. But it is clear that this question must affect to a most important extent the effects of any definite solar motion. For anything that Herschel knew when he began his inquiry, the stars might be so far off that the Sun's motion, even though vast in itself, might produce absolutely no appreciable effect upon the position of the stars. Then again, supposing the first-magnitude stars, presumably the nearest, were affected in a certain degree by the Sun's motion, then the second-magnitude stars would be less affected, the third still less, and so on. Again, the stars regarded as suns resembling our own central luminary, might be expected to have their own motions through space, and it was uncertain before inquiry whether these motions might not be quite sufficient to mask the effects of the Sun's motion; or, at any rate, the balance of effects might be so small as to render it very doubtful whether accident or a real motion of the Sun had been the cause of the slight apparent preponderance of motions in some definite direction.

I mention all these circumstances that the reader may be able to appreciate the boldness of Herschel's genius in venturing to search, amidst so many conflicting evidences as he might expect to meet with, for that small residuum of motion on which he hoped to found the doctrine of the Sun's motion through the sidereal spaces.

But yet more startling is it to find how slender was the stock of materials which Herschel had at his command. When we know that modern astronomers have examined the motions of hundreds of stars in dealing with the same problem, it is amazing to think that Herschel should have hoped to deduce from the motions of only seven stars a result of so much importance as that he was in search of. It is yet more amazing to find that he achieved a perfect success. He announced in 1783 that the Sun is advancing towards a point in the constellation Hercules. Even at the present day astronomers have not been able to say more, so far at least as the direction of the Sun's motion is concerned. Every subsequent inquiry has exhibited the constellation Hercules, or its immediate neighbourhood, as including the point which astronomers call the 'apex of the sun's motion.'

But this result—I mean the successful determination of the direction of the Sun's motion from the consideration of only seven stars—is not only surprising; it is highly significant. Let us at once accept it in its full importance. It proves, not merely that the Sun has such a motion as Herschel had suspected, but that the Sun's motion must bear a very considerable proportion to the motions of the other stars in space. Had the Sun been one of those stars which move very slowly compared with their fellow-orbs, there can be no doubt whatever that so rough and inexact a mode of inquiry could have revealed nothing respecting the direction in which the Sun is travelling. It is only by looking upon the Sun's motion as far from being the least rapid of the stellar journeyings, that we can understand or appreciate Herschel's success.

We shall presently see that this fact is of the greatest significance in reference to the results which have rewarded later researches.

Herschel's conclusions were not left unnoticed or unchallenged. So small, however, has been the number of those who have disputed the justice of Herschel's views, and so thorough has been the vindication which those views have received at the hands of the ablest astronomers, that we need not be at the pains to discuss the arguments by which a few mathematicians (I believe only two) have attempted to impugn the accuracy of Herschel's conclusions.

At an early stage in the inquiry it was felt that the relative distances of the stars must have an important bearing on the determination of the actual nature of the Sun's proper motion. The general drift of the stars in a certain direction may be very significant evidence of the Sun's motion in the contrary direction. But if we wish to estimate the actual velocity with which the Sun is travelling, we must have clear conceptions on two very important points. We must determine the relation between the distances of the brighter and fainter stars, ranging them in definite gradations of magnitude; and, moreover, we must know the actual distance of several stars.

On the last point astronomers have obtained satisfactory results. There are, indeed, very few stars whose distances are known; but several of these distances have been ascertained in a manner there is no disputing. Over and over again, for instance, has the distance of the leading brilliant in the Centaur (a famous double star) been determined; and the results have always been closely accordant. Then there is the small star (No. 61) in the Swan, whose rapid apparent motions long since suggested to astronomers the idea of its proximity to our system. This star's distance has been measured by Bessel and Peters (by independent processes), and with satisfactorily accordant results. And several other stars might be named respecting whose distances very little doubt remains in the minds of our astronomers.

But on the other point there has been a considerable variety of opinion. All agree that the fainter stars must be assumed to lie much farther away than Sirius, or Capella, or Arcturus, or Vega. But whether the relative distance is satisfactorily indicated by the difference of brilliancy, or whether it would not be better, so far as the particular problem we are dealing with is concerned, to take the relative apparent *motions* of the stars as affording the best criterion of their relative distances, is an important point on which astronomers have long been at issue. It is a singular circumstance that they have never (to the best of my knowledge) thought of instituting a direct comparison between the evidence of the two criteria of distance. All that has been done is this. One class of astronomers have arranged the stars into sets according to their apparent brilliancy, and, assigning a suitable mean distance to the stars of each set, have determined the Sun's motion on the strength of the evidence thus deduced. Another class of astronomers have arranged the stars into sets according to the magnitude of their apparent motions, and have determined the Sun's motion accordingly. Now it has happened that the results of both processes have been coincident. Hence it has been assumed to be a matter of indifference which view is adopted, or rather it has been supposed that the relative motion of the stars must correspond pretty closely with their relative brilliancy; otherwise there would be (it has been thought) no such accordance as has resulted from the use of either indication to determine the Sun's motion through space. I speak under correction, but I believe that no one has ever yet expressed any doubts on this point. As I have said, it does not seem to have occurred to any one to inquire whether the proper motions of the stars correspond with the distances which astronomers had deduced from the brightness of the stars of different magnitudes. To illustrate the nature of the case, I may compare it to a trial carried on simultaneously in two different courts and leading to the same decision. In such a case (the trial being carried on with closed doors) no one would be led to suspect that the evidence given in one court might possibly be wholly different from that given in the other, or even that there might be absolute incongruity between the two lines of evidence. Yet this might well be the case; and the decision, however just in itself, might thus be founded on false evidence in one or other, or even in both courts. I think I shall be able to show that the case certainly corresponds with this view in regard to the determination of the Sun's motion through space.

The astronomers who have adopted the rule of estimating star-distances by brilliancy have assigned the following relative distances to stars of the first seven magnitudes:—

	Distance.
1st magnitude	1·00
2nd ,, ...	1·71
3rd ,, ...	2·57
4th ,, ...	3·76
5th ,, ...	5·44
6th ,, ...	7·86
7th ,, ...	11·34

Now it is perfectly clear that the greater the distance of an object in motion, the more slowly the object will appear to move. Just as the apparent dimensions of a man, for instance, are diminished by distance, so also, if he is walking, will the apparent length of his steps be diminished.

This being understood, it is clear that if we take the average proper motions of the stars of different magnitudes, we ought to find a close correspondence between the result and the above list of distances, if the brilliancy of a star affords in reality a true test of its distance. Now, the mode of determining the true average in this case involves mathematical considerations of some nicety. (See Appendix B.) Let it suffice, in this place, to exhibit results.

I find, then, that the average proper motions of the stars belonging to the first seven magnitudes are as follows:—

	Proper motion.
1st magnitude	0″·99
2nd "	0″·21
3rd "	0″·31
4th "	0″·24
5th "	0″·50
6th "	0″·22
7th "	0″·20

And if we estimate the stars' distances from this table, making as before the distance of the first-magnitude stars as unity, we get,* in place of the distances shown in the first table, the following:—

	Distance.
1st magnitude	1·00
2nd "	4·71
3rd "	3·19
4th "	4·13
5th "	1·98
6th "	4·50
7th "	4·95

Clearly this result is unsatisfactory. It is not merely that it differs altogether from the former, but that it is absurd in itself. We cannot suppose stars of the second magnitude to be farther away than stars of the third, fourth, fifth, or sixth. Before proceeding, we must master this difficulty, if possible. I think we can trace it to its source. The first orders of stars contain but few representatives, compared with the orders of the less brilliant stars. Thus the above list deals with only nine stars of the first magnitude, and only fifty-five of the second. It is clear that numbers such as these are insufficient for our purpose, when once it is found that the old law of distance has to be abandoned.

Having thus detected the probable source of the irregularity, it is very easy to recognise the proper mode of treating the difficulty. The question really is whether the faint stars are proportionately far off. Let us then divide the stars into two classes, the first including all those which are visible in moderately bright moonlight—that is, stars of the first three magnitudes; the second including the remaining stars,* visible to the naked eye on a clear night when there is no moon. Such a test as this *must* result in exhibiting the relative average proximity of the brighter stars, if brightness is indeed any criterion of proximity.

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* For a fuller account of my examination of this subject, see Appendix B.

Here is the result:—

	Proper motion.
First set	0"·348
Second set	0"·349

So that, judged in this way, the stars of the smaller magnitudes must be looked upon as actually nearer to us (though very slightly) than the brilliant orbs which form the most striking of our constellations.

One cannot mistake the meaning of this result. The near approach to equality observed between the numbers may be partly accidental. Had our first set included one more magnitude, indeed, there would have resulted no such close approach to equality; but the yet more striking result of a markedly greater proper motion in the fainter set would have been noticed. If our first set had included only the first two magnitudes, there would again have been no exact equality, while the preponderance of proper motion would have been (though but slightly) on the side of the brighter stars. About these niceties we need not concern ourselves. The great point is that it is established, on evidence which seems wholly irresistible, that the brilliancy of the stars is no satisfactory criterion of proximity.

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* It is not to be understood that the stars I am considering include all those of the various magnitudes dealt with. They are taken from a list of the proper motions of 1,167 stars drawn up by Mr. Main in 1864. But it is important to notice that there is no principle of selection in this list to vitiate the result of the process of examination referred to above. All stars belonging to certain catalogues have been included by Mr. Main, whether their proper motions be great, or small, or even evanescent.

This inquiry into the significance of stellar brilliancy may seem a digression from the subject of the Sun's motion in space. But it will presently be seen that the investigation is an absolutely essential preliminary to our inquiry into the recent work of astronomers on the subject of the Sun's motion.

I have mentioned that two methods had been made use of in determining the point in space towards which the Sun is moving. But both these methods were applied to the stars' *apparent motions on the celestial vault.* Lately the Astronomer-Royal suggested a total change in the mode of treating the subject. He argued that the question should be looked at as having reference to the motions of the stars in space, not upon the surface of the imaginary celestial globe. And he showed, in his usual lucid and masterly manner, how the mathematical considerations involved in the change of view must be dealt with. He also calculated the formulæ for determining in this new way the nature of the Sun's motion in space. These formulæ would, of course, be out of place in a popular essay; nor would it be easy to present the new mode of treating the subject without introducing considerations scarcely less unsuited to these pages. The mathematical reader will find Mr. Airy's able dissertation on the subject in vol. xxiv. of the 'Memoirs of the Royal Astronomical Society.' The non-mathematical reader may accept the opinion of our leading astronomical authorities on the merits of this dissertation, as affording sufficient evidence of the justice and importance of the Astronomer-Royal's views. The chief circumstance to be noted, however, is that the new inquiry, though based on novel considerations and conducted in a novel manner, yet led to precisely the same result as former researches.

Not only was the original estimate of the direction of the Sun's motion confirmed by the Astronomer-Royal, but the velocity with which the Sun is journeying through space came out from his figures and formulæ appreciably unchanged. Mr. Dunkin, of the Greenwich Observatory, to whose skill the Astronomer-Royal had entrusted the laborious and difficult processes involved in the application of the new method to 1,167 stars, remarks on this head that the mean of the values obtained by Otto Struve and by himself differs so little from either, that we may look upon it as fairly representing the annual motion of the Sun, 'so far as any result can be obtained by the use of the apparent proper motions of the stars.' The Sun's velocity, according to both methods, is found to be about 150 millions of miles per annum.

It would seem, then, that the problem of the Sun's motion in space has been placed in the category of settled questions. With so many solutions, various in method, founded on different sets of stars, and carried out by the most skilful mathematicians to all but identical results, no one can reasonably doubt, it might be thought, that the Sun's motion is such as has been stated by our astronomers.

And yet, singularly enough, this last and most satisfactory of all the solutions which the problem has received has introduced an element of doubt into the question which it is impossible to overlook. The Astronomer-Royal was not content with the mere solution of the problem, but persisted, after the obstinately inquisitive manner usual amongst astronomers, in applying all manner of tests to the result. Among these tests was one of a most interesting character.

It was argued that if our Sun's motion is reflected so clearly amongst the drifting stars, we ought to find the amount of stardrift largely diminished when the full correction is made for the Sun's motion. Accordingly the experiment was tried. Every star of the 1,167 in Mr. Main's list was carefully set drifting in a direction precisely opposite to that due to the Sun's motion; the effect being intended to be such as to correspond to that which would really take place if the Sun were brought to rest. I say *intended*, because the doubts which hang over the subject of the stars' distances come in here again to perplex the question. However, the best was done which the circumstances admitted of. The estimates of the elder Struve were adopted, and the immensely laborious work of correction was carried out to its completion by Mr. Dunkin.

The result was not such as was to have been expected. I premise that, in this case, the sum of the squares of the uncorrected motions has to be brought into comparison with the sum of the squares of the corrected motions. The former sum, then, was found to be 142·0251, the latter 136·4915. The correction, instead of being important, as was anticipated, is less than a twenty-fifth part of the whole.

Sir John Herschel, commenting on this result, remarks: 'No one need be surprised at *this*. If the Sun move in space, why not also the *stars*? and if so, it would be manifestly absurd to expect that any movement could be assigned to the Sun by any system of calculation which should account for more than a very small portion of the totality of the observed displacements. But what is indeed astonishing in the whole affair is that among all this chaotic heap of miscellaneous movement, among all this drift of cosmical atoms, of the laws of whose motions we know absolutely nothing, it should be possible to place the finger on one small portion of the sum total, to all appearance undistinguishably mixed up with the rest, and to declare with full assurance that this particular portion of the whole is due to the proper motion of our own system.'

With all deference to an authority so distinguished, I must venture to express my doubts of the correctness of the opinion I have just quoted. Sir John Herschel has omitted to consider that the number of bodies affected by their own proper motion may have little to do with the effects of the correction under discussion; because *every one of those bodies is affected by the sun's proper motion.* The largeness of the number is as effective one way as the other. This general consideration suffices to throw Herschel's conclusion into doubt; but in reality the question is one for mathematical discussion, and when this has been applied it becomes certain that his conclusion is erroneous. (See Appendix B.)

But although the processes by which I have established the view that a larger correction was to have been expected are mathematical, I can indicate a line of simple reasoning which will exhibit very clearly the probability of that view. Suppose there are two stars in that part of the heavens where the Sun's motion produces the greatest change of position, and that one star is moving in the same direction as the Sun and with the same velocity, the other in the contrary direction. It is obvious that the first has its apparent motion reduced to rest by the effect of the Sun's motion; the second has its motion doubled. Thus, instead of the motions 1, 1 (as we may count them), which would be observed if the Sun were at rest, we get the motions 0, 2; and instead of the squares 1, 1, we get the squares 0, 4, an increase in all of 2. Now, any one familiar with the elements of geometry will find that, even if the two stars had opposite motions along some other line than the one parallel to the Sun's path, so that (situated as the stars are supposed to be) the squares of their own motions would be less than 1, 1, the increase due to the Sun's motion would still be 2. Hence, if we suppose a large number of stars to be moving in all directions from a point situated as supposed, every pair of opposite motions would have the sum of its squares increased by 2; whereas the original sum would never exceed 2, and would commonly be less than 2. Hence the effect of the Sun's motion would be to more than double the sum of the squares for stars in this particular part of the heavens. For stars situated elsewhere the Sun's motion would be less effective. Without entering further into explanations of the probable effect of the Sun's motion for stars all over the sky, I may mention, as the result of an exact mathematical discussion of the subject, that, on the whole, the Sun's motion ought exactly to double the sum of the squares. In other words, the correction due to the Sun's motion ought to reduce the sum of the squares by one-half. We have seen that the actual reduction is less than one twenty-fifth.

We have, then, only three explanations to choose from. Either the Sun's motion is considerably less than the average motions of the stars; or stars are moving according to some law which tends to mask the effect of the motion of the Sun, which is but one among their number; or, lastly, the assumptions which have been made respecting the stars' distances (as judged by their brightness) are wholly incorrect.

We have seen, at the very beginning of this inquiry, that the Sun's motion cannot be looked upon as small in comparison with the average motions of the stars. Thus the first explanation must be dismissed. As to the second, it is clearly improbable in the highest degree that that should be the real explanation of our difficulty; for besides that a law, to be so effective in masking the effect of the Sun's motion, ought to exhibit itself by obvious relations among the stellar movements, we have the fact that the Sun's motion is *not* masked when estimated in other ways. It is only in one sense that it is masked at all, and its effect, viewed in that light, depends entirely on the assumptions which have been made respecting the stars' distances. We are led, then, to the third explanation, which involves the very conclusion which I have endeavoured to establish above on other grounds—the conclusion, namely, that the distances of the stars are not to be estimated by the stars' apparent brightness.

It only remains to mention that the same amount of change in our estimates of the stars' distances which the former mode of inquiry suggested, appears to correspond closely with the amount required to make the corrections due to the Sun's motions sufficiently effective. I can only speak positively, however, for the stars of the lower magnitudes, because the correction due to the Sun's motion is in reality not a correction at all for stars of the second and third magnitudes, since it actually *increases* the sum of the squares. My formulæ do not enable me to deal with a negative correction, though they show very conclusively that such a correction can only result from mistaken assumptions. The fact that such corrections exist is interesting also as confirming a result already indicated in this paper—viz. that the stars of the first three magnitudes must be taken together in inquiries of this sort.

The result of my examination of the subject seems to prove that we can no longer assume the stars to be all of the same or nearly the same magnitude, but that, on the contrary, their differences of magnitude are so important as to constitute the chief explanation of their differences of brilliancy. But this conclusion, which seems to me indisputable, in no sense contravenes the conclusions which astronomers have formed respecting the Sun's motion through space, though it throws considerable doubt upon the accuracy of the estimates which have been formed of his velocity in miles per annum.

Fraser's Magazine for September 1869.

COLOURED SUNS.

If a brilliant star be observed when near the horizon, it will be seen to present the beautiful phenomenon of 'coloured scintillation.' The colours thus exhibited exceed in purity even those seen in the solar spectrum or in the rainbow. By comparison with them the light which flashes from the ruby, the emerald, the sapphire, or the topaz, appears dull and almost earthy. There are four or five stars which present this phenomenon with charming distinctness. The brilliant Vega in the constellation Lyra, which rarely sets in our latitude, is one of these. At midnight in winter, and earlier with the approach of spring, this splendid steel-blue star may be seen as it skirts the southern horizon, scintillating with red, and blue, and emerald light. Arcturus twinkles yet more brilliantly low down towards the north-east in our spring evenings. Capella is another notable scintillator, seen low down towards the north during the summer nights. But these, though they are the most brilliant northern stars, yet shine with a splendour far inferior to that of Sirius, the famous dog-star. No one can mistake this noble orb as it rises above the southern horizon in our winter months. The vivid colours exhibited by Sirius as it scintillates, have afforded a favourite image to the poets. Homer compares the celestial light which gleamed from the shield and helmet of Diomed to the rays of 'Sirius, the star of autumn,' which 'shines with a peculiar brilliancy when laved by ocean's waves;' and, to pass at once from the father of poetry to our greatest modern poet, we find in Tennyson's 'Princess' the same image, where he says of Arac and his brothers, that—

> As the fiery Sirius alters hue,
> And bickers into red and emerald, shone
> Their morions, washed with morning, as they came.

It is difficult to persuade oneself that these ever-changing tints do not really belong to the stars. But there is now no doubt that they are caused by our own atmosphere. Unequally warm, unequally dense, and unequally moist in its various strata, the air transmits irregularly those coloured rays which together produce the light of a star. Now one colour prevails over the rest, and now another, so that the star appears to change colour. But it is only low down towards the horizon that these changes take place to their full extent. In the tropics, where the air is more uniform in texture, so to speak, the stars do not scintillate unless they are quite close to the horizon, 'a circumstance,' says Humboldt, 'which gives a peculiarly calm and serene character to the celestial depths in those countries.'

But the stars are not wanting in real colours, caused by peculiarities in the quality of the light which they emit towards us. In tropical countries the colours of the stars form a very obvious and a very beautiful phenomenon. The whole heaven seems set with variously coloured gems. In our latitudes, none but the brightest stars exhibit distinctly marked colours to the naked eye. Sirius, Regulus, and Spica are white stars; Betelgeux, Aldebaran, Arcturus, and Antares are red; Procyon, Capella, and the Pole-star are yellow; Castor exhibits a slightly green tint; while Vega and Altair are bluish. Antares, which we have described as a red star, presents, when carefully watched, a greenish scintillation so peculiar as to have early attracted the notice of astronomers. The green tint of Castor had been found to arise from the fact that the star is double, and one of the components green. But, for a long while, powerful instruments failed to exhibit a companion to Antares. At length General Mitchell, with the great refractor of the Cincinnati Observatory, detected a minute green companion to this brilliant red star—the Sirius of red stars, as it has been termed.

But, as we have said, the stars which present distinctly marked colours to the naked eye in our latitudes are few and far between. It is in the telescope that our observers have to seek for a full view of the delicate phenomenon of coloured stars. When a survey is made of the heavens with a powerful telescope, peculiarities well worthy of careful attention are revealed to the observer. We have seen that there are no stars visible to the naked eye which are *decidedly* blue or green. The ancients, also, recognised only red and white stars. In the telescope, this peculiarity is still observable when single stars only are looked at. We meet with some telescopic stars the depth of whose red colour is remarkable. There are stars of a fiery red, of a deep blood-red, and of a full orange colour. There is a well-known star entitled the 'garnet star.' And, in fact, every variety of colour, from white through yellow and orange to a deep almost dusky red, is met with among the single fixed stars. But there is no instance throughout the whole heavens of a single green, blue, or violet star.

The case is altered when we come to examine those double, triple, and multiple stars the observation of which is one of the most pleasing employments of the amateur telescopist. Amongst these systems we meet with all the tints of the rainbow, and with many colours which are not seen in the rainbow, such as fawn-colour, lilac, grey, and so on. 'The attentive observation of the double stars,' writes the celebrated Struve (who detected 3,000 of these objects) 'teaches us that, besides those that are white, all the colours of the spectrum are to be met with.' 'Here we have a green star with a deep blood-red companion, there an orange primary accompanied by a purple or indigo-blue satellite. White is found mixed with light or dark red, purple, ruby, or vermilion.' Sometimes a single system offers at one view many different colours. Such is the case with the remarkable group detected by Sir John Herschel within the Southern Cross. It is composed of no less than 110 stars, which, seen in a telescope of sufficient size, appear, Herschel tells us, like 'a casket of variously coloured precious stones.'

It will be well to examine some of the collocations of colour, that we may trace the presence of a law of distribution, if such exist.

We have said that blue stars are not met with singly in the heavens. Among double stars they are common enough. But they are generally small. When the larger star or primary is not white it is usually either red or yellow; then the smaller star—or satellite, as we may term it—is frequently blue or green. But this is so far from being a law without exception that the more common case is to find both stars similarly tinted. Amongst 596 bright 'doubles,' Struve found 375 whose components were similarly coloured, 101 whose components presented colours belonging to the same end of the spectrum, and only 120 in which the colours were totally different.

Amongst double stars whose components are similarly tinted, by far the greater number are white, yellow, or red. But there are some instances of double blue stars; and in the southern heavens there is a group containing a multitude of stars, *all blue*.

It is impossible, therefore, to suppose that the blue colours seen in multiple systems are due to the mere effect of contrast. In some cases this may happen, however; or at any rate the effect of contrast may intensify the colours of each component of a 'complementary double.' There is one very charming instance of complementary colours in a double star which may be separated with a telescope of very low power. We refer to the star Albireo on the beak of the Swan. The components of this star are orange and blue, the tints being well pronounced. It has been found that when one of the components is hidden the other still preserves its colour, though not quite so distinctly as when both are seen together. Another 'complementary double' is the star γ Andromedæ. The primary is red, the smaller star green. In very powerful telescopes the smaller component is found to be itself double, and doubts exist among astronomers whether the two minute components of the lesser star are both green, or one blue and the other yellow. There is another double star very beautiful in a powerful telescope. This is the star ϵ Boötis, on the Herdsman's belt; it is called also Mirach, and, on account of its extreme beauty, Pulcherrima. The components are nearly equal—one orange, the other a delicate emerald green.

One of the most startling facts revealed by the careful observation of the fixed stars is that their colour is not unchangeable.

We may begin at once with the brightest of the fixed stars—Sirius. This star was known to the ancients as a red star. To its fiery hue may doubtless be ascribed the peculiar influence assigned to it by ancient astronomers. At present Sirius is brilliantly and unmistakably white.

We have not such decisive evidence in the case of any other noted star. But among telescopic stars, there have been some very remarkable changes. There are two double stars, described by the elder Herschel as white, which now exhibit golden-yellow primaries and greenish satellites.

That careful observer, Admiral Smyth, records also that one of the components of a double star in Hercules changed, in twelve years, 'from yellow, through grey, cherry-red, and egregious red, to yellow again.'

The questions may well be asked, Whence do the stars derive their distinctions of colour? and by what processes do their colours change? To these questions modern discoveries have supplied answers which, if not complete, are well worth listening to.

It had long been suspected that the stars are in reality suns. It had been shown that their distance from us must be so enormous as to enable us to assign to them an intrinsic brilliancy fully equal in some instances, and in others far superior, to that of our own Sun. Nothing remained but that we should have some evidence that the kind of light they emit is similar to that which we receive from the Sun. This evidence has been supplied, though only of late years.

We cannot here enter at length into an account of the important discoveries of Kirchhoff and Bunsen, which have enabled astronomers to analyse the light emitted from the celestial bodies. It will be sufficient to remark that in the solar spectrum there are observed fine dark lines breaking the continuity of the streak of light, and that these lines have been proved to be due to the presence of the vapours of certain elements in the solar atmosphere. The proof depends on the exact correspondence of numbers of these lines, grouped in a complex manner (so as entirely to eliminate the possibility of a mere chance accordance) with the bright lines seen in the spectra of light from the vapours of those elements. When once Kirchhoff and Bunsen had proved the possibility of exhibiting the same set of lines either as bright lines on a dark ground or as dark lines on a brilliant spectrum, all doubt as to their meaning in the solar spectrum disappeared at once.

It has been found that in the Sun's atmosphere there are present the vapours of iron, copper, zinc, and nickel, besides calcium, magnesium, sodium, and other metals. But the vapours of tin, lead, silver, and gold do not appear to be present in the solar atmosphere. One of the most remarkable dark lines is due to the presence of hydrogen.

But it has been found possible to extend these researches to the fixed stars. Drs. Huggins and Miller have done this successfully, and their discoveries afford a means of assigning very sufficient reasons for the colours of the brighter stars. By analogy also we may extend a similar interpretation to the colours of stars not bright enough to give a spectrum which can be satisfactorily examined.

Let us take first the brilliant Sirius. This star belongs to the southern half of the celestial sphere, and although it becomes visible at certain seasons in our latitude, it never rises very high above the horizon. In fact, at its highest—that is, when due south—it is only twenty-two degrees above the horizon, or less than one-fourth of the way from the horizon to the point immediately overhead. This peculiarity somewhat interferes with the observation of the star by a method so delicate as that applied by the celebrated physicists we have named. On the other hand the exceeding brilliancy of Sirius makes some amends for the effects of atmospheric disturbances. By selecting very favourable opportunities, Huggins and Miller were able to analyse the star's spectrum, with the following result:—

The atmosphere around Sirius contains sodium, magnesium, hydrogen, and probably iron.

The whole spectrum is covered by a very large number of faint and fine lines, indicating a corresponding variety in the substances vaporised in the star's atmosphere.

The hydrogen lines are abnormally strong as compared with the solar spectrum, all the metallic lines being remarkably faint.

This last circumstance is well worthy of notice, since it is *a peculiarity characteristic of white stars*—so that we begin already to find a hint respecting the source of colour or of the absence of colour in stars.

Take next an orange-red star, the brilliant Betelgeux. The spectrum of this star was very carefully analysed by Huggins and Miller. They marked down the places of two or three hundred lines, and measured the position of no less than eighty. They found that sodium, magnesium, calcium, iron, and bismuth are present in the star's atmosphere, but the two strong lines which note the presence of hydrogen are wanting.

Take next the yellow star, Pollux. The observers were not able to obtain very satisfactory measures of this star; but they established the presence of sodium and magnesium in the star's atmosphere; and again the strong lines of hydrogen were found to be missing.

But we are not entitled to assume that red and yellow stars are characterised by the absence of hydrogen from their atmospheres. On the contrary, the noted red star Aldebaran, the spectrum of which was very carefully analysed by Huggins and Miller, was found to exhibit the two lines of hydrogen with perfect distinctness. This star exhibited a richness in the construction of its atmosphere not presented by any other. The elements proved to be present are sodium, magnesium, calcium, iron, bismuth, tellurium, antimony, and mercury. It must not be supposed, in this or any other case, that other elements might not by a sufficiently laborious scrutiny be proved to exist in the star's atmosphere. The observations required, says Dr. Huggins, 'are extremely fatiguing to the eye, and necessarily limited to the stronger lines of each spectrum.'

It is clear, however, from the above short list of examples, that a considerable variety exists in the physical constitution of the fixed stars. This of itself affords a suggestive hint respecting the true explanation of the variety of colour which we have described. And the peculiarity that in the white stars the hydrogen lines are singularly strong, while the metallic lines are as singularly weak, is yet more to the point. Sirius *was* a red star. Was it at that time unlike present red stars? Does it not seem more probable that, if there had existed in those days a Huggins or a Miller, and the instruments used so successfully by these observers had been invented, it would have been found that Sirius did not —when a red star—present peculiarities now observed only in white stars?

We recognise, then, the influence of time upon the spectrum of this celebrated star, as probably tending to render the lines of hydrogen more distinct than of yore, and the lines of the metallic elements less distinct. But what is the meaning of such a change? Suppose a chemist, for example, observing the spectrum of the flame produced by the combustion of a compound body, should notice that the lines of some elements slowly increased in distinctness, while the lines of others grew fainter, how would he interpret such a phenomenon? If we remembered only that the dark lines are due to the absorptive effect of the vapour they correspond to, on light which is trying, so to speak, to pass through the vapour, we might readily jump at a conclusion, and answer that the extent of absorptive vapour is increasing when the lines are growing more distinct, and *vice versâ*. But we must also consider that these lines are partly the effect of contrast. The lime-light held before the sun's disc appears *black*, though so dazzling when seen alone. It may be, therefore—or rather we may say it certainly is the case— that those parts of the spectral streak which seem dark are in reality luminous; or—which is merely another way of saying the same thing—that the vapours which absorb light from the solar beams, send us light of their own. And so with stars. Therefore, we have this difficulty to contend against,—that there is no power of determining whether a change in the intensity of a line, or of a set of lines, is due to a variation in the light-giving power of the corresponding vapour, or to a variation in the quantity of vapour whose absorptive effects produce the lines.

But, inasmuch as it resulted from Dr. Huggins' examination of a temporary star which appeared last year, that the increase of light—for it was only the abnormal brilliancy of the star which was really temporary—was due to a sudden outburst of inflamed hydrogen, it seems on the whole more probable that the incandescent vapours of stars burn with variable brilliancy, than that they vary in quantitative distribution.

As regards the constant colours of different stars, we are enabled at any rate to deduce negative results.

For instance, we may dismiss at once the theory started some years ago by the French astronomer M. Doppler. He supposed that the colours of a star are due to the proper motions of the star, acting so as—in effect—to lengthen or shorten the waves of light proceeding from the star to the earth, just as the apparent breadth of sea-waves would be greater or less to a swimmer according as he swam with or against their course. It is quite clear that the effects of a motion rapid enough to produce such a change would be to shift the position of the whole spectrum,—and this change would be readily detected by a reference to the spectral lines.*

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* I may be permitted to notice that this was among the earliest *published* references to the possibility of determining motions of recess or approach by the displacement of the spectral lines. Very shortly afterwards, Dr. Huggins had succeeded in applying the method, which he had been endeavouring to do for some months before. I was, however, quite unaware of this when I wrote the above lines. I believe, in fact, his researches were carried on altogether privately.

Apart from this, the colour of a star would not be changed by such motion, the spectrum being merely displaced, not affected in its characteristics of colour. (See p. 275.)

Another theory—that the orange and red tints indicate a lower degree of temperature—must also be dismissed. For we have seen that the spectra of red stars indicate the presence of the vapour of iron and other metals, and nothing but an exceedingly high temperature could vaporise these.

It seems clear that the difference of tint is due to the different arrangement of the dark lines—in other words, to an absolute difference of physical constitution. 'There is a striking difference,' remarks Huggins, 'between the effect on the colour of a star of such closely grouped and very dark lines in the green and blue part of the spectrum of Betelgeux, and of the corresponding part of the spectrum of Sirius, in which the dark lines are faint, and wholly unequal to produce any noticeable subduing of the blue and green rays.'

But we have still to consider the peculiarities presented by the double stars. We have seen that amongst the components of these there are observed some which present a distinct blue colour. It has been found possible to analyse some of these with the spectroscope. We have spoken of the charming double star Albireo, the components of which are orange and blue. Both have been analysed,—with this result, that the spectrum of the orange component is remarkable for the great strength of the lines in the green, blue, and violet, while the spectrum of the blue component is equally remarkable for the great number of groups of fine lines in the orange and yellow.

It would seem, then, that the complementary colours observed in certain double stars, indicate a sort of complementary distribution of elements which in our own Sun are associated equably and intimately.

And we must note here, in passing, that it is not absolutely necessary, as some have supposed, that, if there are systems of worlds circulating around such double suns, there should be any remarkable difference in the quality of light distributed to the planets, as compared with that which we receive from the Sun. Sir John Herschel has spoken of 'the charming contrasts and grateful vicissitudes—a red or a green day for instance, alternating with a white one or with darkness, according as one or other or both of the stars should be above the horizon.' But if the dependent orbs swept in very wide circuits about their double sun, they would receive white light during nearly the whole of each of their days, since it would only be during a brief interval that either sun would be visible *alone* above the horizon.

Of the deeply coloured stars which are visible with the telescope, none have been found sufficiently brilliant to admit of exact analysis.

A peculiarity has been remarked by a distinguished modern observer which is worthy of careful attention. Many of the regularly variable stars, when passing into their phase of minimum brightness, exhibit a ruddy tinge which is very conspicuous in instruments of adequate power. It does not seem easy to explain this as due to any change in the vaporous constitution of a variable star—since it seems difficult to show why such changes should occur at regular intervals. Yet this would appear to be more probable than that these changes are due, either to the rotation of the star itself and the presentation in a cyclic order of the different parts of an unequally illuminated globe, or to the revolution round the star of an extensive vaporous mass whose interposition cuts off from us at regular intervals a portion of the star's light.

It is remarkable that a large number of the known variable stars are red or orange. There is one notable exception, however, for Algol—the celebrated variable in Medusa's head—is a white star.

It is probable that a careful examination of the stars with any efficient 'colour-tester' would lead to the discovery of many cases of variation in colour. Admiral Smyth adopted a chromatic scale of colour—but a test of this sort is not very satisfactory. Opaque colours generally vary with time, so that it is impossible to say that two observers, even if they have used the same strip of coloured discs, have really made observations fairly comparable *inter se.* And it is further to be noted that there are many persons who find a difficulty and uncertainty in the comparison of stars, or brilliants, with opaque colour-scales. An ingenious student of science has suggested the use of chemical solutions, which can always be reproduced with certainty; and he has described a method for forming an artificial star in the field of view of a telescope, and for gradually varying the colour of the star until it should coincide with that of a fixed star whose colour we may desire to determine. The great objection to the plan is its complexity. Coloured glasses, through which a small white disc within the telescope might be illuminated (just as the wires are illuminated in the ordinary transit telescope), would serve the same purpose much more simply.* The inquiry is an exceedingly interesting one, and Sir John Herschel has expressed the opinion that there is no field of labour open to the amateur telescopist which affords a better promise of original discoveries than the search for such variations as we have described.

Fraser's Magazine for January 1868.

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* This plan was proposed by me in the *Quarterly Journal of Science*, for October 1867, in the 'Chronicle of Astronomy.' An instrument of some such sort had been constructed earlier by Mr. Birt, who called it the *homochromatoscope.*

NEWS FROM SIRIUS.

THERE are certain problems in astronomy which have never been satisfactorily solved, though they seem at first sight to present no features of special difficulty, or even to be quite similar in character to other problems which have been found easy of solution. For example, astronomers were for a long time unable to determine the weight of the planet Mercury; and the estimate now accepted is far from being a satisfactory one. Similar difficulties have been encountered in the attempt to estimate the weight of Venus and Mars. Yet these are the nearest of the planets; and Jupiter, Saturn, Uranus, and Neptune, which are so much farther from us, have long since been accurately weighed. We have seen, also,* that the features of Mars—his oceans, continents, and polar ice-caps—have been satisfactorily delineated, whilst those of Venus, our nearest neighbour among the planets, remain altogether unknown. Again, we have learned what elements exist in many of the fixed stars, although the nearest of these bodies is more than 200,000 times farther from us than the Sun; yet we know nothing of the physical constitution of the planets, or even of our near neighbour the Moon.

Amongst other problems which have hitherto appeared insoluble, is that of determining whether the stars have any motion directly towards or from the Earth.

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* See the Essay on Mars, p. 61.

We can form an estimate of the stars' transverse motions, because these result in an apparent change of place. And in the few instances in which we are acquainted with a star's distance, the knowledge of its apparent transverse motion enables us to ascertain the real rate (in miles per year) at which the star is speeding onwards through celestial space. It has been noticed, for instance, that a certain star called 61 Cygni has an annual motion so considerable that in about 350 years the star would be shifted over a space on the heavens equal to the Moon's apparent diameter. Now it happens that this star is one of the few with whose distance from us we are acquainted. In fact, so far as observation has yet gone, this star is nearer to us than any in the northern heavens. Knowing the star's real distance, we can translate the star's apparent motion into real transverse motion in miles per annum. When this has been done, it results that the star is moving over nearly 1,450 millions of miles annually, in a direction at right angles to the line of sight. This motion is equivalent to about 40 miles per second.

But the star may really be moving much more rapidly through space. For, besides this transverse motion, it may have a motion of approach or recession with respect to the Earth. A motion of this sort would, of course, produce no effect on the star's apparent position. The only effect it could have would be to increase or diminish the star's apparent brightness. But so enormous is the distance of the fixed stars that no effect of this sort could be expected to take place. For, let us suppose that 61 Cygni is approaching us at the rate above assigned to the star's transverse motion—that is, at the rate of 1,450 millions of miles in a year. This space, enormous as it seems, scarcely exceeds the fifty-thousandth part of the star's distance; so that in a thousand years the star would not be nearer to us by more than one-fiftieth part of its present distance.

It seems, therefore, quite hopeless to look for information respecting any motions of this sort among the fixed stars. For, if no evidence of motion towards or from us can be detected in the case of a body which is certainly one of the nearest among the fixed stars, it is still less likely to be afforded in the case of other stars.

Yet the problem here presented is precisely the one whose solution we have to record. The manner in which the problem has been solved is deserving of careful study. We shall have to make some preliminary remarks, which at first sight seem scarcely to bear on the subject we are dealing with.

It is known that light travels in a series of waves of extreme minuteness, and propagated with extreme velocity through an ethereal medium which occupies all space and the interstices of solid bodies. We know little of the habitudes of this ethereal medium; in fact, we only know of its existence through its quality of transmitting light and heat. So long as light and heat were supposed to travel directly from the Sun and stars to the Earth, the existence of a fluid occupying the interstellar and interplanetary spaces could hardly have been suspected. But the case is different now that the undulatory theory of light has been established. For, just as the transmission of the tidal wave from the Southern Ocean to our own shores is an evidence (and would be, of itself, a sufficient evidence) that the waters which wash our shores communicate with the southern seas, so the fact that light-waves from the Sun and from the stars reach our Earth, affords sufficient evidence that the medium in which they travel occupies, without break or interruption, the interplanetary and interstellar spaces.

The waves of light are, as we have said, exceedingly minute. It has been proved that their average length is about the fifty-thousandth part of an inch. But they are not all of the same length; and light-waves of different length produce light of different colours. There are some light-waves so long as the forty-thousandth part of an inch: waves of this length produce red light. There are others so short as the sixty-thousandth part of an inch: waves of this length produce violet light. Waves of the average length produce green light. And we may remark, in passing, that this is doubtless the reason why green light is so agreeable to the eye; for the light-appreciating powers of the eye are called into fuller exercise in dealing with waves belonging to either extreme.

It is not to be supposed that there are sudden limits to the length of the waves we are dealing with. Just as there are sounds which are too grave or too acute to be appreciated by the ear, so there are light-waves, or rather, we should say, there are forms of light which the eye has no power to appreciate *as* light. Such waves produce effects—heating, actinic, and chemical—but the eye does not recognise them as light-waves.

Light travels at the rate of 180,000 miles per second, and the question may here arise—and will be found to have an important bearing on the subject of our paper—whether waves of different length travel at the same rate. This question must be answered, it should seem, in the affirmative. For, since light takes nearly an hour in travelling from Jupiter to us, it would follow, if there were any appreciable difference in the rate at which the longer and shorter light-waves travel, that the satellites on emerging from eclipse would not appear white. Suppose, for instance, that the longer light-waves travelled fastest, then a satellite immediately after eclipse would appear red, and gradually, as light of the other colours of the spectrum came to reinforce the red light, the colour of the satellite would change from red through orange, buff, fawn-colour, and flushed white to pure white. Similarly, if the shorter light-waves travelled fastest, the colour of the satellite would change from violet through indigo, olive, russet, and greenish-white to pure white. As no such changes occur, we may assume with considerable confidence that light-waves of different length travel at the same rate.

We now have to consider a circumstance which may be aptly illustrated by the waves of the sea. If we imagine a stout swimmer urging his way amidst a wave-tost sea, or, rather, amidst a sea crossed by a succession of long rollers, we shall see that, according to the direction of his motion, he would be apt to form a different estimate of the rate at which the waves were travelling. It is clear that in the case, only, of his swimming in a direction parallel to that of the wave-fronts, would the waves seem to pass him at their true rate. If he swam facing them they would seem to travel more quickly, and if he swam with them they would seem to travel more slowly, than they would if he were at rest. Now, if he were not to consider his own motion, he would be led by these varying appearances to form varying estimates, not merely of the velocity of the waves, but of their *breadth*. The faster the wave-crests passed him, the narrower would the waves appear to be, and *vice versâ.*

It is obvious that similar considerations apply to any system of waves whatever. Take, for instance, the waves in air which produce sound. These travel at the rate of nearly 1,200 feet per second. If a sound be maintained at a given *pitch*—that is, by air-waves of given length—this sound will appear to vary in pitch according as the auditor is at rest, or moving towards or from the source of sound—if only, in the latter cases, the observer's rate of motion bears an appreciable proportion to the rate at which sound travels. It was stated by the late Professor Nichol of Glasgow that the experiment has actually been tried. 'On the railway uniting Utrecht with Maarsen, were placed at intervals of something upwards of a thousand yards three groups of musicians, who remained motionless during the requisite period. Another musician on the railway sounded at intervals one uniform note, and its effects on the ears of the stationary musicians have been fully published. From these certainly—from the recorded changes between *grave* and the more *acute*, and *vice versâ*, confirming even *numerically* what the relative velocities might have enabled one to predict—it appears justifiable to conclude that the general theory is correct, that the note of any sound may be greatly modified if not wholly changed by the *velocity of the individual hearing it*,' *or*, he should have added, by the velocity of the source of sound itself.

Let us apply the same consideration to light-waves. We must first consider the velocity of light. It will appear, at first sight, hardly conceivable that any orb in the celestial spaces should be moving with a velocity bearing an appreciable relation to the enormous velocity with which light travels. Even the velocity of 61 Cygni—about 40 miles per second—would almost be *rest* as compared with a velocity of 180,000 miles per second. We may compare the relation between these unequal velocities to that between the velocity of the swiftest express train and a velocity of about 20 yards per hour, or 1 foot per minute—a velocity scarcely exceeding that of the snail. If, therefore, we supposed the star 61 Cygni *to shine with light having a constant wave-length*—in other words, with *monochromatic* light—we could not expect to detect any difference in the colour of its light on account of any motion the star may have towards or from the earth.

But a consideration connected with the words we have italicised renders the solution of our problem in this way altogether hopeless. Returning to our swimmer, if waves of every possible length between certain limits were passing him, and he were only capable of noticing those which seemed to lie between much narrower limits of length, it would clearly make no difference whether he swam with or against the course of the waves. And this case exactly corresponds with that of the observer on Earth. The astronomer, M. Doppler, who first suggested that the colours of the stars, and especially of certain double stars, might depend on the stars' motions of recession or approach, omitted to take this important circumstance into consideration. If we assume that a star were approaching us so rapidly that the waves of red light were apparently reduced in length so as to produce the effect of orange light, then the orange part of the star's light would produce the effect of yellow light, the yellow of green, the green of blue, the blue of indigo, the indigo of violet, and lastly the violet part of the light would become inappreciable. So far, then, there seems to be a change—in the loss of all the red part of the light. But as it is certain that there are light-waves of greater length than those which produce red light, and that these waves by being apparently shortened could become appreciable to the sight and give the effect of red light, we see that there would be absolutely no change whatever in the colour of the light received from a star moving towards us even at the tremendous rate indicated by our supposition.

Thus we seem to be no nearer the solution of our problem than we were before.

But there is a peculiarity in the light received from the Sun and stars which remains to be mentioned, and which has led to a very satisfactory and trustworthy solution of the difficult problem we have been dealing with.

It has been observed that the solar spectrum is crossed by a multitude of dark lines parallel to each other and at right angles to the length of the spectrum. These lines are arranged in so complex a manner that each of the stronger lines, and every group of faint lines, is distinctly recognisable. Thus physicists speak of the strong line F in the green part of the spectrum, of the double line D in the orange part of the spectrum, of the group of seven lines in such and such a part of the spectrum, and so on. These lines never vary in arrangement or position. Corresponding lines are seen in the spectra of the stars; the spectra vary among themselves, but each spectrum remains constant as respects the arrangement of its distinctive lines. But note also that, although different stars have different spectra, yet these variations arise merely from the fact that certain lines are present in one spectrum and wanting in another, or *vice versâ*. The lines which *do* appear are the same lines which have been measured in the solar spectrum. Thus a physicist will say—In the spectrum of such and such a star the lines B, D, and F are well seen; the existence of C and E is suspected, but these lines are very faint; G and H are not seen. He *knows* that these lines are the same as those in the solar spectrum, either because he has carefully estimated their position, or because he has brought the star's spectrum into direct comparison with the spectra of certain terrestrial elements in which these lines appear.

Now here we have at once a most delicate means of detecting stellar movements of approach or recession. If in the spectrum of a star we can see a recognisable group of lines, or a line recognisable by its strength, and if in any way we can prove that this line does not hold the exact position which it has in the solar spectrum, then the change of position must be looked upon as due to the star's motion towards or from the Earth. The shifting of the spectrum bodily, which, as we have seen, produces no change whatever in the star's *colour*, brings all the *lines* into new positions, and any one line, marked enough for ready examination, suffices as well as a hundred to determine the existence of such a change.

We need hardly say, however, that the inquiry, even under these favourable circumstances, is one of extreme delicacy. In the ordinary prismatic spectrum the change of position would be wholly inappreciable, and the eminent physicist who has just succeeded in solving the problem in the case of the star Sirius, had to make use of a spectroscope having a dispersive power seven times as great as that of a single equiangular prism of crown glass, in order sufficiently to magnify the variation in question. This gentleman, Dr. Huggins, came to the examination of the problem we are considering, with a large amount of experience in spectroscopic researches; yet it was a problem of such extreme difficulty that much time was expended and many experiments were made before he could conduct his inquiry to a successful issue.

Dr. Huggins first satisfied himself that a certain conspicuous line in the spectrum of Sirius corresponds to the line F in the solar spectrum. This line also appears as a bright line in the spectrum of the light of hydrogen. The spectra of Sirius and of incandescent hydrogen were then brought side by side for direct comparison. With the powerful dispersing spectroscope made use of by Dr. Huggins, the line F in the spectrum of Sirius was found to be separated by about one two-hundred-and-fiftieth part of an inch from the corresponding line in the spectrum of hydrogen. The displacement was towards the red end of the spectrum, so that it indicated a motion of *recession* between the Earth and the star.

Now the displacement having been measured very accurately, we are enabled to calculate the rate at which Sirius is receding from the Earth. The observed alteration is found to indicate a recession at the rate of 41·4 miles per second. But we must consider the Earth's motion also, because she moves so rapidly around the Sun as largely to affect the apparent motions of recession or approach which the stars may have with respect to her. She travels around her orbit at a mean rate of about eighteen miles per second. At the time of Dr. Huggins's observation the direction of the Earth's motion was such that she was receding from Sirius at the rate of about twelve miles per second. Deducting this velocity from the total rate of recession, it results that Sirius is receding from the Earth at the rate of about 29½ miles per second, or about 930 millions of miles annually.

Two circumstances have to be considered, however, before we can look upon the actual motion of Sirius as determined.

It has been calculated that the Sun, with its system of attendant orbs, is speeding through space at the rate of 150 millions of miles per year. And it happens that the point in space towards which the Sun is moving—which lies in the constellation Hercules—is almost exactly opposite the constellation Canis Major in which the star Sirius is situated. Therefore we must diminish the above-mentioned motion of recession by nearly the whole amount of the Sun's proper motion, leaving to Sirius *a proper motion of recession of about* 780 *millions of miles per annum.*

Lastly, we must consider the transverse proper motion of Sirius. It follows from Henderson's estimate of the distance of Sirius (lately confirmed by the researches of Mr. Cleveland Abbe) that the star has a transverse motion of about 450 millions of miles per annum. Combining this motion with the star's motion of recession, we deduce an actual velocity through space of upwards of one thousand millions of miles in a year, or about thirty-three miles per second.

But it is rather from what is promised than from the information which has actually been obtained, that the process of inquiry so successfully pursued by Dr. Huggins derives its chief interest. Doubtless the discovery that the brightest star in the heavens is speeding onward with so enormous a velocity through space is in itself well deserving of our attention. But if it shall become possible—and we see nothing in the nature of things which should prevent it—to determine in the same manner the motions of recession or approach of all the stars visible to the naked eye, then we shall have a fund of knowledge from which many most important facts respecting the economy of the stellar system cannot fail to be deduced.

For, let us consider the nature of the knowledge which astronomers had already gleaned respecting stellar motions, and the use to which they had applied that knowledge.

They had obtained exact estimates of the apparent motions of the stars—or what is termed their proper motion—upon the celestial sphere. But, at first sight, these estimates appear almost valueless, so far as our views respecting the true motions of the stellar universe are concerned. For, first, as we have already mentioned, the motion thus indicated in any case might in reality be but a small portion of a star's true motion. And further, unless a star's *distance* be known, the determination of the proper motion affords no indication whatever, even respecting the star's true transverse motion. Now there are not twenty stars in the whole heavens whose distances from us have been estimated *in any way*, and there are not ten whose distances can be said to have been satisfactorily determined. Nor is there much probability that the list will ever be greatly extended. For, the distances of the fixed stars are so enormous that the powers of our best instruments and the skill of our best observers are taxed to the utmost to obtain—even in a few favourable instances—any information whatever respecting the minute and almost evanescent shifting of position on which the determination of a star's distance depends.

And yet from the consideration of the imperfect information afforded by the stars' apparent proper motions, astronomers have been able to deduce one of the most interesting astronomical discoveries yet effected. They have learned that the Sun with his attendant system is speeding onwards through space, in a certain direction which they have been able to assign, and at a rate of no less than 150 millions of miles per annum. A law also, affecting the general system of stellar motions, has been *guessed at*, and has been considered by many eminent astronomers to be supported by sufficiently satisfactory evidence. It has been supposed that the proper motions of the stars indicate a vast series of orbital motions around a point in space which does not lie very far from the star Alcyone—the principal star of the Pleiades. I am not putting forward this supposed law as standing by any means on a similar basis with the fact of the Sun's onward motion through space. Indeed, I think that the researches on which the law has been founded are far from being sufficient to establish such an hypothesis. But what I wish to dwell upon is the circumstance that the observed proper motions of the stars, imperfect as is the evidence they afford, have yet led to the discovery of one important fact, and to the attentive consideration of a yet more important law of stellar motion.

But now, if the method which Dr. Huggins has begun to apply should be extended to all, or even to a large proportion of the fixed stars, what important conclusions may we not hope to see deduced from such observations. For, in the first place, the motions of the stars directly towards or from us are quite as significant as their transverse motions; secondly, we shall know more about the former motions than we have ever been able to learn about the latter; and lastly, neither kind of knowledge considered separately could possibly lead to such satisfactory results as we may hope to gather from the knowledge of the *actual* motions of the stars through space. There now really seems a promise that one day something may come to be learned respecting the movements of the sidereal mechanism. The constellations which now seem to be scattered without discernible law over the vault of heaven may be forced, perhaps, to reveal to us their secrets, the law of organisation which binds them into a system, the paths along which their component stars have been travelling before they reached their present position, and those along which they are to travel for many future ages. Meantime long processes of patient labour and systematic observation lie before the astronomer. Not in our day, nor perchance for many generations, will the Copernicus of the stellar system appear; and for him astronomers will have to lay up during those long years a rich store of materials. 'How much,' says Sir John Herschel, 'is escaping us! And how unworthy is it in those who call themselves philosophers, to let the grand phenomena of nature—those slow but majestic manifestations of the power and glory of God—glide by unnoticed and drop out of memory beyond reach of recovery, because we will not take the pains to note them in their unobtrusive and furtive passage, because we see them in their everyday dress, and mark no sudden change, and conclude that all is dead because we will not look for signs of life, and that all is uninteresting because we are not impressed and dazzled. To say indeed,' he adds, 'that every individual star in the Milky Way is to have its place determined and its motion watched would be extravagant; but at least let samples be taken—at least let monographs of parts be made, with powerful telescopes and refined instruments—that we may know what is going on in that abyss of stars, where at present imagination wanders without a guide.'

Fraser's Magazine for October 1868.

EQUAL-SURFACE PROJECTIONS OF THE GLOBE.[*]

In this paper the term 'projection' is to be understood in the sense used in mapping—viz. to signify any method of construction by which the meridians and parallels of a map may be laid down.

Since it is impossible to represent any portion of a globe without distortion, the map-maker seeks to satisfy the conditions which seem most important for the particular purpose he may have in view. Thus he may propose to himself the construction of a map in which distances measured from the centre, or along a latitude-parallel, shall be correctly given, as in the *equidistant* projection, and in Flamsteed's projection, respectively; or in which errors of distance shall be distributed as equally as possible; or he may aim chiefly to obtain correctness of shape, either in large figures—a problem approximately solved in the *conical* projection, or in small figures, a problem completely solved in the *stereographic* and *Mercator's* projections, and which may be solved in an indefinite number of ways; or, again, he may propose that certain lines on the globe shall be represented by straight lines in the map—as great circles and rhumb-lines, for instance, are represented in the *gnomonic* and *Mercator's* projections respectively. These and many other problems may be proposed, and maps constructed to fulfil such conditions will have a special value for special purposes. The problem I now propose to examine is that of the construction of maps in which equal areas on the globe shall be represented by equal areas on the map. M. Babinet, who first proposed such a construction, called it the *homolographic* projection of the globe; the term *isographic* seems preferable, however.

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[*] The following essay, though not strictly astronomical, is introduced here as illustrating the method of charting which I have used in discussing the laws of stellar and nebular distribution.

It is stated in Nichol's 'Cyclopædia of the Physical Sciences,' that Cauchy, the celebrated mathematician, solved Babinet's problem, though it is not easy to see what difficulty Babinet could have found, since the problem admits of many simple solutions. I am unable to say whether Cauchy's solution corresponds with any of those I am about to indicate.

The advantages of isographic projection for special purposes are obvious. Maps thus constructed are not necessarily much distorted; but of course when the whole of the sphere is represented in a single projection, as in the figures which illustrate this article, the distortion is very great in parts of the map.

The method illustrated in Fig. 15 results from the solution of the following problem:—Two neighbouring latitude-parallels being taken, including between them a very narrow belt of surface, required to find a point on the polar axis from which this belt would be projected into a ring of equal area on the north-polar tangent-plane. The solution of this problem gives a formula from which it results that each such belt must be projected from a different point;* in other words, that there is no single point for which any finite area of the globe can be isographically projected. But the complete projection can be obtained in an elegant manner by a sort of double (true) projection. Imagine a hemispherical shell of twice the radius of the sphere standing on the centre of its curved surface upon a horizontal table; let the sphere be placed within the hemisphere, the north pole of the sphere coinciding with the point of contact of the hemisphere and table, so that the axis of the sphere is vertical, and the south pole coincides with the centre of the hemisphere. Now suppose both the sphere and the hemispherical shell to be transparent; the meridians, continent-outlines, &c., on the sphere opaque; and that a luminous point placed at the south pole of the sphere casts shadows of the opaque lines upon the hemispherical shell; then, if these shadows be traced in opaque lines, and the luminous point be removed vertically upwards to an indefinite distance, the lines traced on the hemisphere will be *orthographically* projected as shadows upon the plane table. Now it is easy to show that any small area on the sphere is increased by the first process, and the increased area diminished in the same proportion by the second process, so that the area of the final projection is equal to the true area on the globe—in other words, the final projection is *isographic.*

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* The formula is $x = 2r\left(1 + \cos\frac{\lambda}{2}\right)$ where r is the radius of the sphere, λ the mid-latitude of the belt, and x the distance of the point of projection for the belt, from the north pole of the sphere. The plan had already been described by Sir John Herschel in his noble work, *Observations made at the South Cape.* I was not aware of this, however, when I wrote the above lines.

The construction for this projection is simple. If the meridians and parallels are to be drawn to every tenth degree, proceed as follows:—Describe a circle with a radius equal to twice that of the globe to the scale of which the projection is to be drawn; divide the circumference to every fifth degree; draw a pencil-line from the centre to one of these divisions, and a series of other pencil-lines—which will cross the first at right angles—connecting divisions equidistant from the first on either side of it; describe circles concentric with the first through the points in which the first straight line is crossed by the others—these are the *parallels*; and

FIG. 15.

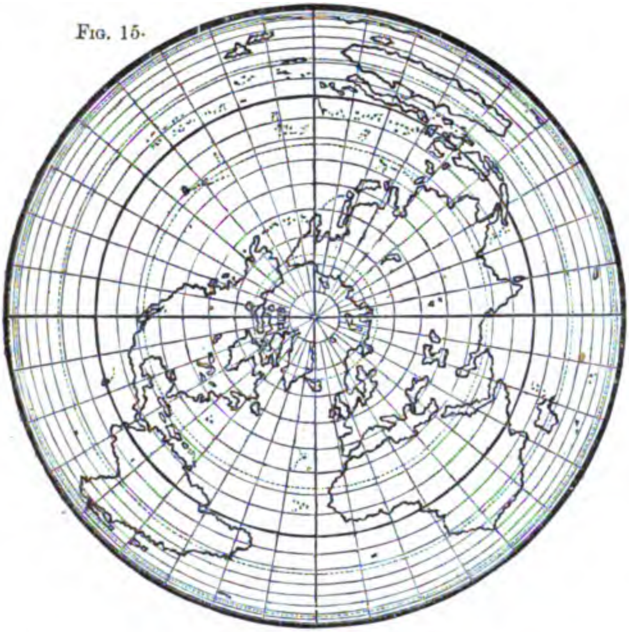


FIG. 16.

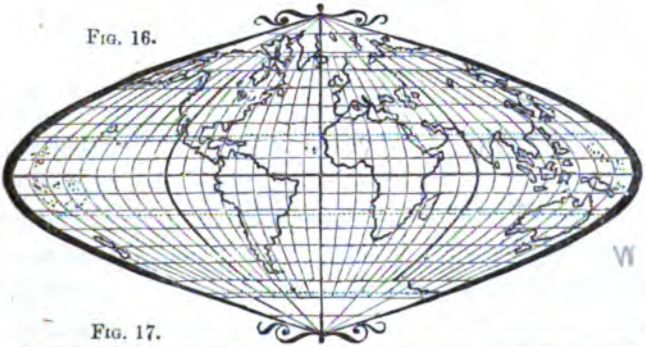
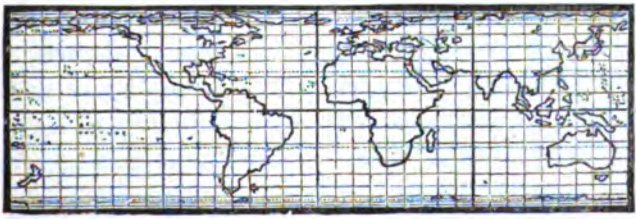


FIG. 17.



Figs. 16 and 17, scale two-thirds of Fig. 15.

THREE EQUAL-SURFACE PROJECTIONS OF THE ENTIRE GLOBE.

lastly, draw straight lines from the centre to alternate divisions round the outer circle—these are the *meridians.* In other words, if we omit alternate meridians in the *polar orthographic* projection of the meridians and parallels of a *hemisphere* to every *fifth* degree, we have the meridians and parallels of the *polar isographic* projection of a *complete sphere* to every *tenth* degree. We may now darken the middle parallel, which represents the equator, mark in the tropics and arctic circles in their proper places, as shown by the dotted circles, and draw in the continents and islands according to their proper longitudes and latitudes. It is convenient also to darken two meridians at right angles to each other; for this purpose we may select the meridian separating the old and new hemispheres (so called) from each other, and the meridian at right angles to the former. In other words, the darkened meridians and parallels of Fig. 15 (as of the other figures) correspond with the circumferences and the horizontal and perpendicular diameters of the maps of the two hemispheres commonly given in our atlases.

The method illustrated in Fig. 16 is an extension of Flamsteed's projection to the whole globe. The construction is simple: a series of equidistant parallels represent the parallels of latitude, a perpendicular cross-line representing a meridian bisects all the parallels, which are made equal in length to the actual parallels on a globe of the scale of the figure, the distance between them being also equal to the true distance separating successive parallels on such a globe. The parallels being divided into equal parts, corresponding points of division are connected by curved lines representing the meridians, as shown in Fig. 16, in which meridians and parallels are laid down to every tenth degree. It is obvious that this projection is equigraphic, for the spaces near the central meridian represent the corresponding spaces on the globe both in size and shape, and all the spaces between any pair of parallels are equal, though they vary in shape, for each may be divided into two unequal triangles, and we see that the greater and less triangles of any one space are equal to the corresponding triangles of another, since they have equal bases, respectively, and lie between the same parallels.

The last method is founded on the property that if a sphere is enclosed in a cylinder, any two planes parallel to the base of the cylinder enclose between them equal belts of surface of the sphere and cylinder. Now, suppose that the polar axis of a globe is the axis of the enclosing cylinder, and that this axis is luminous, but can only emit rays of light at right angles to its own length; then, if the meridians, continent-outlines, &c., are opaque and the sphere transparent, shadows of these lines will be cast on the enclosing cylinder, the points on each parallel of latitude being projected in the plane of their parallel, owing to the supposed peculiarity of the luminous axis. If the cylinder be now opened along a line parallel to its axis and unrolled, we shall obtain the isographic projection represented in Fig. 17. The construction is simple. Taking a horizontal cross-line to represent the equator, and therefore equal in length to the circumference of the globe, we divide it into equal parts and through the points of division draw perpendiculars representing the meridians; these must be equal in length to the diameter of the globe, and must be bisected by the equator. On the outside meridians describe semicircles (in pencil), and divide their circumferences into half as many equal parts as the equator was divided into, and through corresponding points of division draw parallels to the equator; these represent latitude-parallels, and in Fig. 17 meridians and parallels are drawn to every tenth degree.

There are many methods by which the globe may be isographically projected; the three just described are, however, the simplest in construction, and, in some respects, the best that can be devised. The qualities of these projections, and the purposes which maps formed upon them are best suited to fulfil, are different.

In the first projection we have a very accurate map of the north polar regions; the northern hemisphere is presented without great distortion, but all the outlines of the southern hemisphere are much distorted. The projection can, of course, be applied to the southern quite as well as to the northern hemisphere; however, as the greater portion of the land lies north of the equator, the northern is obviously the proper pole for the centre of the projection, if only one map is to be drawn. It is not advisable to present the northern and southern *hemispheres* in two maps, since one of the chief advantages of this as of most polar maps, is that it indicates the connection between the different parts of the globe. It would be well, however, supposing maps of this sort were to be introduced into atlases, either to give two maps of the complete sphere, or at least to supplement the north polar map with a map of southern regions from the pole to the tropic of Capricorn.

Such a map as Fig. 15, on a suitable scale, would form a useful addition to works on physical geography. Consider, for instance, the following extracts from Humboldt's 'Cosmos:' 'The superficial extent of dry land, compared with that of the liquid element, is as 1 : 2·8, or according to Rigaud, as 100 : 270.* The islands form scarcely one twenty-third part of the continental masses, which are so unequally distributed that the northern hemisphere contains three times as much land as the southern, which is pre-eminently oceanic. . . . . The liquid element predominates equally in the space comprised between the eastern shores of the old and the western shores of the new continent, where it is only interrupted by a few scattered groups of islands. . . . The southern hemisphere and the western, from the meridian of Teneriffe, are therefore the most oceanic portions of the globe.' 'The two great insular masses or continents—eastern and western, old and new—present some striking contrasts, and, at the same time, some analogies worthy of notice. Their major axes are in opposite directions;' the eastern extending from east to west, the western from north to south, roughly. 'On the other hand, both continents are terminated towards the north, nearly on the seventieth parallel; and towards the south run into pyramidal terminations, having submarine prolongations indicated by islands and shoals.' Some of these remarks, it will be noticed, are satisfactorily illustrated by a map on Mercator's projection, others by any map on an isographic projection; but the polar isographic projection is the only satisfactory illustration of the complete series of remarks.

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* According to others, 100 : 284.

I may notice, in this connection, a remark of Humboldt's, which illustrates the singular liability even of the best observers to error when intent on establishing a favourite view. Humboldt, in labouring to establish the prevalence of law in the figuration of the continents, adduces the following in stance:—'We perceive,' he says, 'that the terminations of the three continents, *i.e.* the southern extremities of Africa, Australia, and America, successively approach nearer to the South Pole;' apparently not noticing that three promontories round a globe must necessarily exhibit such successive approach, either in an easterly or westerly direction. In fact, the arrangement actually observed is exactly that which seems least indicative of law, since the points of Africa and America, which are those separated by the shortest difference of longitude, are also those which differ most in latitude. A law might seem to be suggested if Africa extended to a southern latitude intermediate between the latitudes limiting Australia and South America, or (in a less degree) if South America extended to a latitude intermediate between the latitudes limiting Australia and Africa. In either case, however, the certainty that the three points must lie in order would be sufficient to weaken any evidence founded on the different distances (in longitude) between them. Humboldt adds, that 'New Zealand seems to form a regular intermediate member between Australia and South America.' But if New Zealand is to be considered in such an inquiry, then Madagascar, which is larger than all the New Zealand islands together, must also be considered; and the assumed law is not followed in the case of Madagascar. If it be argued that Madagascar is clearly only a 'submarine prolongation' of Africa, it may be answered that it is quite as clear that Australia itself, and all the islands of the Australian Polynesia, are only submarine prolongations of the Asiatic continent (see Fig. 15).*

Such a projection as that of Fig. 15 is especially adapted to illustrate works on the physical geography of the sea, since the correct presentation of areas is obviously important in illustrating the subjects treated of in such works; and it is also important that currents, tidal wave-fronts, tracks of ships, &c., should be presented without the breaks which occur in maps on Mercator's projection. There are many passages, for instance, in Maury's 'Physical Geography of the Sea' which seem to require the illustration afforded by maps on the polar isographic projection. I will select one on a subject of some interest. In this case the illustration afforded by the north polar map is such as to throw doubt on conclusions, which, judged by Mercator's chart, seem sufficiently plausible :—

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* It may be noticed that the mathematical chance of four promontories round a globe forming a regular progression, either in an easterly or westerly direction, is $\frac{1}{3}$rd, of five $\frac{1}{12}$, of six $\frac{1}{60}$, and so on; the chance of n promontories forming such a progression, in either way, being $\frac{2}{\underline{|n-1}}$, where $\underline{|n-1}$ means the continued product of the natural numbers from 1 to $n-1$. In other words, the 'odds' against these respective events are 2 to 1, 11 to 1, 59 to 1, and $(\frac{1}{2}\underline{|n-1}-1)$ to 1, respectively.

In the earlier editions of this interesting work Captain Maury had expressed the opinion that there exists 'a basin of considerable extent in the Frozen Ocean, supplied by water coming in at the bottom, and rising up at the top, with a temperature not below 28°, the freezing point of sea-water.' He cited in evidence the facts that 'whales have been taken near Behring's Strait with harpoons in them bearing the stamp of ships cruising on the Baffin's Bay side of the American continent;' that 'icebergs high above the water,' and therefore extending more than seven times as far below the surface, 'have been seen drifting rapidly northwards through Davis' Strait against a strong surface current,' * showing that there is a powerful under-current towards the North Pole; that De Haven saw a 'water-sky' northward and westward from Wellington Channel; and, lastly, that the birds and animals of arctic regions migrate *northwards* in search of milder climates. This last circumstance, by the way, does not afford such strong evidence as at first sight it appears to do, since, owing to the peculiar disposition of the autumn and winter isotherms, a course very nearly northerly would in many cases be the proper course for arctic birds and animals seeking the nearest spots combining a suitable climate with the conditions suited to the existence of these creatures. For instance, the shortest course from Parry Islands to Spitzbergen, with its comparatively mild winter climate, would pass very near the North Pole. However, it may be considered as established, that (in summer certainly, and possibly in winter) large open seas exist in the arctic regions.

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* Even in midwinter. Thus Captain Duncan, of the whale-ship 'Dundee,' writes—'December 18th (1826). It was awful to behold the immense icebergs working their way to the north-east from us, and not one drop of water to be seen: they were working themselves right through the middle of the ice.'

But in treating of this subject in later editions, Captain Maury has gone much further, as the following extract shows: 'Dr. Kane reported an open sea north of the parallel of 82°. To reach it his party crossed a barrier of ice 80 or 100 miles broad. . . . . Passing this ice-bound region by travelling north, he stood on the shores of an iceless sea, extending, in an unbroken sheet of water, as far as the eye could reach, towards the North Pole. Its waves were dashing on the beach with the swell of a boundless ocean. The tides ebbed and flowed in it, and I apprehend that the tidal wave from the Atlantic can no more pass under this icy barrier to be propagated in seas beyond, than the vibrations of a musical string can pass with its notes a "fret" upon which the musician has placed his finger. . . . . *These tides, therefore, must have been born in that cold sea, having their cradle about the North Pole; and we infer that most, if not all, the unexplored regions about the Pole are covered with deep water; for were this unexplored area mostly land, or shallow water, it could not give birth to regular tides.*'

It may be true, though it seems far from probable, that a vast arctic ocean occupies all the unexplored regions near the North Pole: but the 'birth of tides in that cold sea' is certainly not established by the evidence adduced. Referring to Fig. 15, the reader will see, to the left of the central dark meridian, a strait, opening into the arctic regions within the eightieth parallel. It was here that Dr. Kane saw an open sea extending as far as the eye could reach to the north and north-east, with land (the most northerly yet seen) in the north-west. About as far to the right of the dark meridian, and just south of the eightieth parallel, is the island of Spitzbergen; above, on the next dark meridian, is Nova Zembla; beyond that again, Liakhov Island, about as far from Behring's Strait (between Asia and America) on the Asiatic side as are the Parry Islands on the American side; and next to the Parry Islands is the strait from which we started. Now it is necessary, for the purposes of Maury's argument, that it should be proved that there is no communication between the place reached by Dr. Kane and the Atlantic Ocean on the one side, or the Pacific Ocean (through Behring's Strait), on the other; in other words, that a land-locked or ice-bound ocean surrounds the North Pole, and that this ocean is so large that the attractions of the Sun and Moon on *different parts* of it are *sufficiently unequal* for the generation of tides.* Now it seems clear to me that even if the whole of the unexplored regions about the Pole were covered by a deep land-locked ocean, yet no appreciable tides could be generated in an ocean so situated. But an arctic (land-locked) ocean must be much more contracted; for Captain Parry sailed on an open ocean between Liakhov Island and the Parry Islands, and Captain M'Clure proved that sea communication exists between Behring's Strait and the Parry Islands. Therefore any land or fixed ice locking the supposed arctic tidal ocean on this side must lie within 10° or 12° of the North Pole. But a far stronger argument against Captain Maury's view may be derived from the results of Sir E. Parry's celebrated sledge excursion from Spitzbergen; for although Parry *saw* no open sea north of the eighty-first parallel, yet his expedition proved the *existence* of open sea as far north as the eighty-fifth parallel (at least); and therefore not only contracted to this extent the space within which a land-locked arctic ocean (if any exist) must be confined, but established the probability that open sea communication exists between the Atlantic and the point reached by Dr. Kane.

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* It is clearly only by such inequality of attraction that tides can be generated in a land-locked polar sea.

Let us see how these results may be deduced from the very circumstance which led to Parry's failure. Starting from Spitzbergen in the boats 'Enterprise' and 'Endeavour,' the expedition sailed northwards for three days; but ice gradually gathering round them, they commenced a 'boat and sledge' journey over a vast body of ice. After five weeks of labour, during which they travelled 290 miles northwards, they found that the distance from the starting-point was only 172 miles, the *southerly drift of the whole field of ice* over which they had been laboriously tracking their way having carried them back 118 miles! They had reached latitude 82¾° north, and they could see no sign of clear water in any direction. Now, remembering that an experienced arctic sailor can detect a 'water-sky' long before open water can be seen, we may safely assume that the field over which they had travelled so far extended fully fifty miles farther north, and as far to the east and west. But this is not all. The point they had reached must have been 118 miles farther north when they started, since the whole field had travelled southwards by that amount. Therefore the open sea *communicating with the Atlantic*, on which their vast ice-ship was *floating freely*, had, when they started, extended at least 168 miles further north than latitude 82¾°, or further north than the eighty-fifth parallel (half-way between Spitzbergen and the Pole—see Fig. 15). It was precisely in this direction that Dr. Kane saw open water as far as the eye could reach—say twenty or thirty miles. The fact that so large a field, probably extending at least 100 miles from east to west, drifted freely for 118 miles, seems to prove that the sea on which it floated is deep, for otherwise islands or shallows would probably occur which would check the motion of large fields of floating ice. In fact, the voyages of Hudson (1607), Buchan and Franklin (1818), and Scoresby (1806) had sufficed to establish the probability that a vast offshoot of the Atlantic valley extends far towards the North Pole; and it was in this sea, in latitude 76° north, that Dr. Scoresby sounded to the depth of more than a mile and a half without finding bottom. It seems, therefore, far more probable (to say the least) that open sea communication exists in summer between the point reached by Dr. Kane and the great sea that washes the shores of Iceland and Spitzbergen—in other words, that Greenland is an island—than that the tides seen by Kane were born in a land-locked arctic ocean, and had their cradle near the North Pole.

There are several purposes for which the projection of Fig. 16, independently of the advantage it possesses as an isographic projection, seems better suited than Mercator's. For instance, it would exhibit isothermal lines, lines of equal mean barometric pressure, and other such lines, more correctly both as respects their length, and as respects the distances between successive lines.

The projection of Fig. 17 has the advantage of simplicity of construction, and the map being divided into rectangular areas, it is easy to determine, by a few simple measurements, the areas of continents, countries, oceans, or seas.

Any other point might be adopted for the centre of the projection of Fig. 15, but the construction for the meridians and parallels would no longer be simple. The same remark applies to the projections of Figs. 16 and 17, if any other great circle than the equator is adopted for the central line; but of course any other meridian may be assumed as the central meridian without varying the construction.

One other point remains to be noticed. If the distances between successive meridians were uniformly diminished in any of these projections, so that Fig. 15 became a sector instead of a circle, and Figs. 16 and 17 were uniformly contracted horizontally on both sides of the central meridian, the projections would still be isographic. For special purposes such changes may be useful. Some arrangements of Fig. 15 may be noted in particular. If, instead of a complete circle, a semicircle be divided into thirty-six equal angles, the parallels remaining unchanged, the spaces near the equator will have very nearly their true figure, and the distortion near the South Pole will be diminished by one-half. This semicircle can be formed into a cone by bringing together the bounding radii (considering the semicircle as a sector), and the connection between different parts of the globe can thus be indicated as clearly as in Fig. 15. But a still better representation of the globe may be formed by taking two equal sectors of about three right angles, dividing each into thirty-six equal angles, describing parallels up to the equator, and drawing in the continent-outlines of the northern hemisphere in one sector, and of the southern hemisphere in the other. If the two sectors be formed into cones,* and these joined along the equator, an isographic representation of the globe on a double cone will be formed, the vertices being the Poles. The construction of these figures, or of others formed in a similar manner from sectors of varying angles (or constructed on other projections), would form an interesting and instructive employment for the young geographer.

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* The vertical angle of either cone will be a right angle, if the angle of either sector is 254½°. In this case the axis of the double cone will be equal in length to the diameter of the equator, and the distortion will not be very great in any part of the projection. In the case first considered (of a semicircle) the vertical angle of the cone would be 60°.

A NOVEL WAY OF STUDYING THE STARS.

THE celestial depths have been studied during many ages, and by means of various instruments. They have been mapped and charted and guaged, and the laws according to which the stars seem to be distributed have been carefully studied with the object of determining the real arrangement of these orbs throughout space. But so far as I am aware it has never yet occurred to astronomers to apply to the study of the heavens two simple and perhaps insignificant instruments, by means of which I have lately been endeavouring to elucidate the subject of stellar distribution. I think I may safely claim to be the first who has sought to interpret the secrets of the heavens with the aid of a pair of scissors and a trustworthy balance.

Lest the reader should be tempted to dismiss at once all consideration of such trivial methods of research, let me remind him, in passing, that these simple instruments have already been employed to solve an important problem of terrestrial physics. All our books on geography speak of the proportion between the oceans and the continents of our Earth, but the simple contrivance by means of which this proportion has been determined is not so commonly mentioned. On a reference to Humboldt's *Cosmos* it will be found that the method of procedure was as follows:—From the strips of paper intended to cover an ordinary terrestrial globe the parts representing land were carefully cut out with a pair of scissors; they were then placed in a delicate balance and weighed; the same was next done with the remainder of the paper—that is, the part representing oceans. The relation between land and sea surfaces followed at once from the relation between the observed weights of a few seemingly insignificant scraps of paper.

It had long since occurred to me that a similar method might be applied to the strips of paper intended to cover a celestial globe. One could thus determine the relative richness with which stars are spread over certain regions of the heavens, or ascertain the actual extent of such regions as the Milky Way, the great nebular districts, and so on.

But there was one objection to the plan. It was rather costly. We can readily obtain a copy of Messrs. Malby's Star-Atlas, in which the strips for covering a globe are included; but when we have this valuable work we are scarcely disposed to cut up the beautifully engraved maps. Nor, again, is it easy when a number of small portions of the heavens are thus presented in different maps to complete the process of dissection by which the extent of different regions is to be determined. And then there is not, as in the case of a terrestrial globe, one question only to solve; there are half-a-dozen at least, and each would involve the sacrifice of a fresh atlas.

Fortunately, there is a way of getting over this difficulty.

It is possible to map a hemisphere—nay, the whole heavens if we will—on a plane surface, in such sort that every space on the heavens shall cover its true relative proportion of the map. This mode of mapping is not at all suitable for the ordinary purposes which maps are intended to subserve; for this true representation of superficial proportion is secured only by introducing a very marked distortion of the *shapes* of different regions. Everyone knows how strangely distorted the British Isles are in an ordinary map of the eastern hemisphere; but these isles would be even more singularly shaped in maps on the particular projection I am now referring to.

It is clear, however, that mere distortion is of no importance where one seeks only to determine the relative dimensions of different regions. Most fortunately too, the plan of projection is a very simple one, and the constructions involved are remarkably easy.*

It will be seen at once that by such a contrivance one can determine the relative dimensions of any part of the celestial sphere without sacrificing an atlas or a globe. It is necessary first to map down the two hemispheres, each on a large sheet of good paper uniform in texture. Then all the paper outside each circular map must be cut away, and each map weighed. Of course the two maps would be of equal weight if each sheet were of the same thickness. But large sheets of drawing paper, even though of the same general quality, often differ appreciably in weight; and it is well, in such researches as I am describing, to weigh each map separately, so that afterwards one can compare the weight of any part with the weight of the map from which it has been cut.

Let it be noted, in passing, that this plan can be adopted for testing the relation between land and sea surfaces; and it is probable that more trustworthy results would be obtained by carefully drawing the northern and southern hemispheres on such a projection as I have described, and cutting out continents and islands, than by applying the same process of section to the numerous strips required for covering a globe.

Let us now consider the various astronomical questions which the process of weighing may help to answer.

During the last few years I have been led to recognise several peculiarities in the distribution of the stars visible to the naked eye, peculiarities which appear to be opposed to the ordinarily accepted theories respecting the sidereal system. For example, there is a tendency among such stars to associate into streams and groups, and clustering aggregations, covering, perhaps, several constellations, or at least distinguished, by the extent of space they cover, from such clusters as the Pleiades or the Beehive in Cancer. Then, again, there are regions of the heavens where stars are sparsely distributed, lying side by side sometimes with other regions in which stars are spread with unusual richness. One other peculiarity of this sort is in an even more marked manner opposed to those ordinarily accepted views according to which the lucid stars form but, as it were, the threshold of the great sidereal system guaged by the Herschels. Over the Milky Way, whose light, according to those views, comes from multitudes of stars lying at distances immeasurably exceeding those of the lucid stars, the latter yet seem spread so much more richly than over surrounding regions as to suggest the idea that in truth they are immersed amid the groups of faint stars whose united lustre produces the milky light of the galaxy. It is obviously a matter of great moment to determine whether this relation is apparent only or real; since if it be real it follows inevitably that the faint stars in the Milky Way are for the most part really small and not merely faint from vastness of distance.

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* There are several methods of drawing a map of a complete globe, in such a way that all spaces shall be presented of their true relative dimensions. See preceding essay on Equal-Surface Projections.

And here, at the very threshold of the inquiry, a problem presented itself for solution which my new method—the 'scissors and balance method' let it be called—was competent to solve very easily. I think there is no work on astronomy in which the actual extent of the celestial sphere occupied by the Milky Way is referred to, or in which any estimate of this extent is given. It is clear that by ordinary methods the determination of the surface of so strangely complicated a zone as the galaxy, with its convolutions and broken branches, and those islands of light and lakes of darkness pictured in Sir John Herschel's *Southern Observations*, would be a matter of great difficulty. But it is an exceedingly easy matter to cut out from a map the parts marked down as belonging to the galaxy, and to weigh those parts against the rest of the map.

In this way I found that the southern half of the galaxy covers one-eleventh of the southern hemisphere, while the northern half of the zone covers one-tenth of the northern hemisphere. I had anticipated a different result, since the southern part of the Milky Way seems to cover a larger space than the northern; but repeated trials led always to the same result; and when a tracing of the two half-zones had been made on a single sheet of paper, the northern portion was always found to outweigh the southern. The fact is that the remarkably wide range of the Milky Way over the southern constellations Scorpio and Sagittarius is more than compensated by the extensive *lacunæ* in this region.

It follows that the whole of the galaxy covers between one-tenth and one-eleventh of the heavens. This I believe is the first estimate that has ever been formed of the proportion.

To turn, however, to the express object of the new mode of research—the determination of the relative richness of stellar distribution in different parts of the heavens.

It was necessary in the first place to mark in all the stars visible to the naked eye. There is no perfect list of such stars, but the British Association Catalogue of 8,316 stars is supposed to contain all but a very few of the lucid stars. Removing the telescopic stars from this list—that is, all the stars below the sixth magnitude—there remain 5,850 stars of the first six orders. To mark in, in their proper places, so many as 5,850 stars is a work requiring much time, if carried out directly from the catalogue itself. Considering that the maps of hemispheres would have no value at all except for the particular inquiry I was upon, the labour involved would have been somewhat disproportioned to the antecedent probability that the results would be of interest. But for this, and my very limited leisure, the results I am about to present would have been obtained three or four years ago. The recent completion of my atlas, however, rendered it possible for me to mark in the 5,850 stars on the projections of the two hemispheres much more easily and quickly than if I had worked directly from the catalogue.

Having completed this work, I placed each map upon six large sheets of drawing paper, of uniform quality and texture, and pricked off all the stars upon the six sheets at once. These pin-hole maps were to be cut to pieces, and their fragments weighed. It will be noticed that, as no ink was used in marking the stars, there was no addition to the weight of the paper, though I by no means assert that the inquiry was one of such extreme delicacy that the additional weight of the ink-spots would have appreciably affected the result.

The first process of weighing and counting was applied to the Milky Way. I was particularly anxious to test the truth of a view which I had put forward long since, that the lucid stars are much more thickly strewn on the Milky Way than over the rest of the heavens. In most works on astronomy the contrary is asserted. Sir John Herschel's clear vision had not failed, however, to recognise the peculiarity, and accordingly we find, in his 'Outlines of Astronomy,' the expression of an opinion that the lucid stars *do* congregate somewhat more thickly than elsewhere, upon and near to the Milky Way. It is worthy of notice, though, that in his wonderful work on the 'Southern Heavens' (a perfect storehouse of astronomical facts) he mentions as the result of a systematic process of inquiry, that the lucid stars do *not* congregate on or near the galactic zone, and that, in fact, the condensation of stars on that region begins only to be marked and obvious after we have passed the ninth and tenth orders of star-magnitude. It is very important to notice this seeming discrepancy. Both opinions are perfectly just. If we treat the galaxy as a zone, taking a uniform band of the heavens along the region covered by the contorted and complex streams of the Milky Way, we do not find any signs of greater aggregation of lucid stars either on the zone itself or in its neighbourhood; and yet, looking at the Milky Way as it actually appears in the heavens, we do find signs of such aggregation. It is because the aggregation follows the contortions and complexities of the Milky Way itself, while the gaps and *lacunæ* are left relatively clear of lucid stars, that both the statements made by Sir John Herschel are justified. The richness of stars on the Milky Way is counterbalanced by the sparseness of their distribution close by; so that when we take a uniform zone covering the Milky Way and also its gaps and *lacunæ*, we fail to recognise either the richness of one region or the poverty of the other. The indications which might guide us to important conclusions are placed completely out of view by the process of taking averages: the irregularities of the field we are working over and surveying are smoothed away just where it is most important that they should not be lost sight of.

Now let us see what the new method tells us as to this interesting subject of inquiry.

We have seen that the southern half of the Milky Way covers one-eleventh part of the southern hemisphere. It contains no less than 618 lucid stars. So that if the whole heavens were strewn with equal richness there would be twenty-two times 618 lucid stars, or 13,596 instead of the

5,850 actually observed. The northern part of the Milky Way covers one-tenth of the northern hemisphere, and contains 497 stars; so that if the whole heavens were strewn with equal richness there would be twenty times 497 stars, or 9,940 instead of 5,850.

Taking the whole of the Milky Way, we find no less than 1,115 stars in a space covering between one-tenth and one-eleventh of the celestial sphere. So that if the whole heavens were spread as richly with stars there would be about 11,650 stars, or almost twice as many as are actually observed.

But now a very singular result has to be noticed. In the dark gaps and *lacunæ* in the Milky Way there are very few lucid stars indeed. There is no reason, according to accepted views, why lucid stars should avoid these gaps; on the contrary, they might be expected to be spread rather richly so near the galactic zone; but, as a matter of fact, they are wanting there. I weigh the 'coal sacks,' as the *lacunæ* are termed, and also the space between the two streams of the Milky Way where it is double, and I find that these regions cover in all about one sixty-second part of the whole celestial sphere. Upon them there are found but twenty lucid stars. So that if the whole heavens were not more richly strewn with stars, there would be but 1,240 visible stars instead of 5,850.

It surely is a most significant fact that, whereas the Milky Way itself is so thickly strewn with lucid stars that the whole heavens, spread as richly, would contain 11,650 stars, the *lacunæ* and gaps are so poverty-stricken that, were the celestial sphere as poorly spangled with stars, we should see but 1,240, or less than one-ninth the former number. To understand the meaning of this result, the reader must remember that, according to accepted views, there is no antecedent reason whatever for expecting that the absence of that nebulous light which comes from telescopic stars should involve an absence of lucid stars. The telescopic stars are supposed to be very much further off than the lucid orbs; so that such an association as I have described is as surprising and unexpected as though the clouds on a summer sky should seem arranged in lines and cross-lines, corresponding to the bars and sashes of a window through which we viewed those clouds.

We have only to read Sir John Herschel's words having reference to the supposed proof that no such relation exists, in order to feel the full significance of the now established fact that the relation is exhibited in a very marked manner. Sir John Herschel, by taking zones over the Milky Way, instead of the Milky Way itself—a perfectly just course, be it remarked, if only the accepted views are just—found, as I have mentioned, that 'the tendency to greater frequency, or the increase of density in respect of statistical distribution, in approaching the Milky Way, is quite imperceptible among stars of a higher magnitude than the eighth' . . . and that 'it is with the eleventh magnitude that it first becomes conspicuous.' He then proceeds (still reasoning very justly so far as the imperfect evidence he was dealing with is concerned):—

'Two conclusions seem to follow inevitably from this; viz. first, that the larger stars are really nearer to us (taken *en masse*, and without denying individual exceptions) than the smaller ones. Were this not the case, were there really among the infinite multitude of stars composing the remoter portions of the galaxy, numerous individuals of extravagant size and brightness, as compared with the generality of those around them, so as to overcome the effect of distance and appear to us as large stars, the probability of their occurrence in any given region would increase with the real apparent density of stars in that region, and would result in a preponderance of considerable stars in the Milky Way, beyond what the heavens really present, over its whole circumference.

Secondly, that the depth at which our system is plunged in the sidereal stratum constituting the galaxy, reckoning from the southern surface or limit of that stratum, is about equal to that distance which, on a general 'average, corresponds to the light of a star of the ninth or tenth magnitude, and certainly does not exceed that corresponding to the eleventh.'

Both these conclusions are inevitably controverted by the evidence given by the new method of inquiry. For it is to be noticed that there is no question as to which result (of two which seem contradictory) should be preferred. The new method not only supplies the just result, but explains how the erroneous result has been obtained. We have only to apply the new method to a zone over the Milky Way instead of to the actual convolutions of that stream, to obtain precisely the same result as that on which Sir John Herschel founded the two conclusions described above.* In this way, not only the

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* It may be as well, perhaps, for me to point out that in thus commenting with perfect freedom on the views of the most eminent astronomer of our day, one whose works I have read again and again with ever-increasing delight and instruction, I take the course which seems to me at once the most straightforward and the most respectful. It is no compliment to our great men to express objections to their views in a tone implying that they value their own opinions more than truth. It has chanced—very much to my surprise—that I have been somewhat severely, and, as I think, very unjustly, rebuked by a well-known student of astronomy for so plainly discussing the views of others. To me this criticism is not merely objectionable—it is wholly unintelligible. There has never lived a man of science, however eminent—not Newton himself, nor Bacon, nor Humboldt, nor Laplace—who has not adopted erroneous views. In the nature of things it must be so. To men of this stamp—to a Herschel, an Airy, or an Adams—far more than to men of inferior attainments, the idea of being always in the right appears, indeed, egregious in its absurdity. As facts are accumulated, it is as absolutely certain that the views of our great men will have to be in part or wholly modified, as that science must be progressive and not stereotyped. That, under these circumstances, it should be thought offensive to exhibit the bearing of new truths upon views admittedly formed on imperfect evidence, and to do this in the simplest and most straightforward manner possible, seems to me preposterous. Certain I am that our great men have no reason to thank any who would speak of them as though their fame were a plant of tender growth which the breath of free discussion would wither.

It may be suggested that new views can be put forward without mentioning *lacunæ* and gaps have to be added, their poverty counterbalancing *pro tanto* the richness of the Milky Way, but the singularly bare regions bordering on the Milky Way near Aquila and Cygnus, and from Auriga past the feet of Gemini to Monoceros, have also to be included. It is plain that by thus combining poor and rich regions together, we get an apparently average degree of richness for the whole zone. We smooth away, as I have said, the very irregularities from which we have the best chance of detecting the true laws of the sidereal system.

The inevitable inference from the evidence obtained by the new mode of research is, that there is some association between the clusters of minute stars which produce the milky light of the galaxy, and the lucid stars which aggregate so richly amid those clusters. The apparent association is too marked not to warrant us in believing that a real association exists. Those minute stars are not then far out in space beyond the lucid stars, as had been supposed: they are, therefore, not so large as the lucid orbs, but owe their faintness to real relative minuteness.

There is but one escape from this conclusion—the possibility, namely, that the observed arrangement is only due to an extraordinary coincidence. I shall presently have occasion to exhibit the overwhelming nature of the antecedent probabilities against such coincidences, and I think I shall be able to convince the reader that, even as respects a single instance, no such explanation would be available, while the peculiarities of distribution which remain to be considered

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the views of others. This may seem—to those who have never had occasion to put forward new views—to be the case. But on trial the reverse appears. It not only seems most uncourteous to present new views without mentioning the contrary opinion of eminent authorities, but it leads a large number of readers to conceive that the new views are simply put forward in ignorance of the fact that this or that authority had adopted other opinions.

place any interpretation by a reference to mere chance-distribution altogether out of the question.

Several years since I had noted the existence of star-streams outside the Milky Way, the components of the streams being stars of the first five orders of magnitude. Many of these streams are sufficiently obvious to have attracted the attention of astronomers in long past ages—insomuch that in the old maps of the heavens the nature of these streams was supposed to be adequately represented by the introduction of the river Eridanus, the winding serpent Hydra, the band between the Fishes, and the streams from the water-can of Aquarius. In the southern heavens the prolongations of some of the star-streams are singularly marked. I ventured to entertain the view that, contrary to the opinions hitherto expressed, these star-streams are not to be explained as due to mere accident—to the apparent association of stars at very different distances and not actually associated—but are really streams of stars in space.

It was objected (not unjustly) by the Savilian Professor of Astronomy, that these streams can be 'made to go anywhere:' one observer might opine that a stream extended in a very marked way in one direction, while another would recognise its real prolongation along an array of stars lying in quite another direction.

There is not the least doubt that this is the case. In fact the star-streams might be more justly described, in many instances, as star-reticulations; and, like all reticulations, these can be traced out in many different ways. This, however, by no means proves that the association of stars into these streamy reticulations is due to mere chance distribution.

But it was further urged that, if the streams are real, they ought to be more and more marked the farther we extend the process of charting—that is, the lower the orders of magnitude included in our maps. Here, I must admit, I was not at one with the objectors. It seemed clear to me that, on the contrary, the introduction of stars of the lower orders of magnitude might be expected to destroy all signs of orderly arrangement. For it must be remembered that, according to my views, the sixth-magnitude stars include a number of orbs which are in reality small compared with the stars of higher orders, and also a number of stars which are faint only through excessive distance. And therefore it was clearly to be anticipated that the commingling of two sets of stars so perfectly dissociated from each other would result rather in rendering the evidence perplexed and confusing than in throwing any new light on the question of the star-streams. Again, even supposing that nearly all the sixth-magnitude stars were really smaller than those of the leading orders, and were intimately associated with these, it by no means followed that the signs of such association would result in making the star-streams more marked. To take a parallel instance: if we consider the greater branches of a tree, we recognise at once the branch-shaped figure of the spaces they occupy; but if we consider the smaller branches as well, we find that the general shape of the space occupied by the tree is no longer branch-like; and if we extend our consideration to the leaves, and so come to regard the tree as a whole, we recognise the space it occupies as anything but branch-shaped, though the branches really exist all the same. In like manner if we study the river system of a continent, we find the regions traversed by the great streams to be winding and contorted, and in some sort resembling the streams which flow through them. But the regions fed by the complete river-system of a continent are not stream-shaped, for in fact they constitute the continent itself. So that it might very well happen, or rather it was to be expected, that the regions over which stars down to the sixth magnitude are spread with exceptional richness would not resemble streams at all, but (seen as they are—near and distant portions alike—projected on the background of the celestial sphere) would only be recognisable as great clustering aggregations extending over several constellations.

It was with some confidence that such a result would appear that I mapped the 5,850 lucid stars contained in the British Association Catalogue. But there were reasons for feeling doubtful. No evidence had hitherto been obtained on the subject. Any one who studies the heavens themselves instead of maps will see at once how difficult it is to form any clear conception of the laws according to which the fainter orders of lucid stars are distributed. Nor were any maps in existence from which clearer conceptions could be formed.

As the work progressed I found marked signs of special laws of aggregation. These laws were of an unexpected character. Besides the aggregation of stars along the Milky Way, already referred to, I found that two roughly circular regions exist, one in the northern and one in the southern hemisphere, within which stars are spread with greater richness than elsewhere. The southern rich region is further remarkable, first on account of its extent, and secondly as surrounding, but not quite concentric with, the larger of the two Magellanic Clouds. The northern is smaller, and its centre lies near a singular projection of the Milky Way in the direction of the Lesser Bear.

Within a week of the completion of the maps, I was able, in a lecture delivered at the Royal Institution, to assert definitely that these rich regions exist, though at that time I had not had leisure to construct maps suited for the application of the method discussed in this paper.

Now let us examine the results obtained by the new method of research.

The rich northern region covers the constellations Cepheus, Cassiopeia, Lacerta, and portions of Cameleopardalis, Cygnus, and Draco. It contains 622 stars, and the 'balance' shows that it covers rather more than one-seventh of the northern hemisphere (more exactly, three twenty-second parts). If covered with equal richness the whole heavens would contain more than 9,050 stars instead of 5,850.

The rich southern region is much larger. It covers, in fact, exactly half the southern hemisphere. The centre of the region lies between the Southern Pole and the greater Magellanic Cloud, so that the borders of the region lie farther from the South Pole, towards Eridanus and Argo, than on the opposite side towards Scorpio and Sagittarius. This region contains no less than 2,467 lucid stars, the surrounding part of the southern hemisphere, though fully equal to the rich region in extent, containing but 893 stars, or little more than one-third the number contained by the rich region. So that if the whole heavens were bespread as richly with stars as the latter region, there would be 9,868 visible stars; whereas if they were bespread no more richly than the surrounding regions, there would be but 3,572 visible stars!

But what makes this result even more remarkable is the fact that the poverty of the outer region is referable to two portions lying along the very borders of the rich region. Where the Milky Way crosses the outer region there is no exceptional poverty, but (as might be inferred from what has been already stated respecting the Milky Way) a relative richness of distribution. It is where the borders of the rich region cross Cetus and Eridanus, and again on the opposite side, where they approach Hydra, that the poorest regions of the southern heavens are to be found.

In order to render these contrasts more obvious, I cut out so much of the richest portion of the rich southern region as covered the same extent as the small northern rich region.*

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* It is convenient in these researches to deal with regions of equal extent; and obviously the new method lends itself very readily to this requirement,

I further cut out the two very poor regions just referred to, taking so much of them as covered the same extent. I thus had three portions of the southern heavens, each (as measured by weight) of equal extent; but one richly, the other two sparsely strewn with stars. On counting the actual number of stars (or pin-holes) I found that the first contained 895 stars, while the two latter contained severally but 161 and 216. So that stars are spread more than five and a-half times as richly over the first as over one of the poor regions, and more than four and a-half times as richly as over the other.

The very rich region here selected extends over the keel of the ship Argo from Canopus to Crux, and northwards towards the Greater Dog. It is of this region that the well-known astronomer Captain Jacob remarked that when it had risen above the horizon the effect of its light could be recognised, even when the eye was not directed towards it, precisely as though a young moon had risen. But it was the glory of the Milky Way here, and of the brilliants Canopus and Sirius, that was supposed to occasion this effect, not the richness with which stars of the lower lucid orders are spread over the region.

In the northern heavens the poorer regions are somewhat more diffusely distributed. But there are two long and somewhat straggling regions, one extending from Corona and Serpens over Bootes to Leo, the other extending from the Dolphin, over Pegasus, the head of Andromeda, and Pisces, towards Orion, which are not less poverty-stricken than the two southern regions described above. Taking regions here of the same actual extent* (measured by their weight as

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since one can snip round the borders of a region until its size has been reduced —as measured by its weight—to that of another region.

[*] For this purpose it was necessary to carry the former region slightly into the southern hemisphere, the overlapping portion extending to but not trenching

before), I find that the former contains 175 stars, the latter 201. It will be remembered that the rich northern region, though covering no greater space, contains 622 stars, or more than three and a-half times as many as one, and more than three times as many as the other.

If the above results be presented in a tabular form, their full significance will be recognised. For symmetry I add a northern region corresponding to the large southern region, though not covering quite half the northern heavens. It includes the rich northern region, and in addition part of Andromeda, Perseus, the Cameleopard, the Lion, the Greater Bear, the Northern Crown, Hercules, and Lyra. It contains 1,420 stars, and covers five-elevenths of the northern hemisphere; and in the following table is referred to, like the great southern region, as the central region of its proper hemisphere. The numbers in the second column represent the total number of visible stars which would appear over the whole heavens, if stars were as richly strewn throughout as in the particular regions referred to in the first column:

	Relative richness of distribution.
Northern—Milky Way	9,940
„ Richest region	9,050
„ Central region	6,248
„ Outer region	3,923
„ 1st Poor region	2,948
„ 2nd Poor region	2,567
Gaps in Milky Way	1,240
Southern—1st Poor region	2,361
„ 2nd Poor region	3,198
„ Outer region	3,572
„ Central region	9,868
„ Richest region	13,126
„ Milky Way	13,596

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upon one of the southern poor regions. It is noteworthy that dividing the heavens into two hemispheres is unfavourable to the completeness of the contrasts above insisted upon. I hope soon to have leisure to prepare two equatorial hemispheres on a projection of similar properties, to correct this defect. Yet the evidence as it is is sufficiently convincing.

But it may be argued that, after all, these results are perhaps merely due to chance distribution; very strange combinations have been known to result accidentally—why need any peculiarities in the distribution of the stars be insisted upon as necessarily pointing to the existence of special laws of aggregation? Since there are several thousand stars, it would be strange (an objector might urge) if some more or less remarkable peculiarities did not present themselves.

It is precisely the largeness of the number of stars dealt with by the new process which assures us that the observed peculiarities are not due to accident.

To take one of the less remarkable peculiarities noted above. We have in the northern and southern central regions twice as many stars as in the remaining regions. Now the former do not between them cover one-half of the heavens; but let us, for the sake of simplicity, assume that they do. And, still farther to simplify matters, let us suppose that there are exactly 6,000 visible stars, of which 4,000 fall within this particular half of the heavens, and 2,000 in the remainder. This closely corresponds to the observed case, and differs from it only in a sense unfavourable to my argument.

Now supposing 6,000 points are to be spread at random over a globe, it is quite clear that, as each point is marked in, the chance that it will fall on any given half of the heavens is exactly equal to the chance that it will fall on the other half. So that we might measure the chance of such a state of things as we have actually found to exist, by comparing it with a very commonplace process—the tossing of a coin. We have only to inquire what the chance would be that, on tossing a coin 6,000 times, there would be at least as many as 4,000 heads (or tails).* Now it might seem that, since it is no unusual thing to see as many as four heads or tails in tossing a coin six times, there would be nothing very surprising were 4,000 heads or tails to appear in 6,000 tossings. The actual chance of such a result is so inconceivably minute, however, that all ordinary modes of representing it fail me. Supposing there were a bag with a million white balls and but one black ball in it, the chance of drawing the black one at random would be thought sufficiently minute. But to represent the chance of tossing either heads or tails 4,000 times at least in 6,000 trials, we should require a bag exceeding in volume many million times the whole of the sidereal system as far as the most powerful telescopes have yet penetrated, and this bag must be filled with white balls so tiny that a million of them would not be as large as a pin's head, only one black ball (as minute as the others) being present throughout its whole extent. The chance of drawing that one tiny microscopic black ball out of that universe of white ones would exceed the chance that in any given set of 6,000 tossings there would be 4,000 heads (or tails) at least.*

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* Some consideration would have to be given to the fact that the rich regions are *selected*, were it not that their roughly circular form is quite sufficient to

It will be conceived, therefore, how unspeakably minute is the chance that the observed relation among the fixed stars is due to mere chance distribution.

We are forced then to accept as the legitimate conclusion from the evidence, the theory that within the limits occupied by the lucid stars there are streams and clustering aggregations of stars, and not that general uniformity assumed in the

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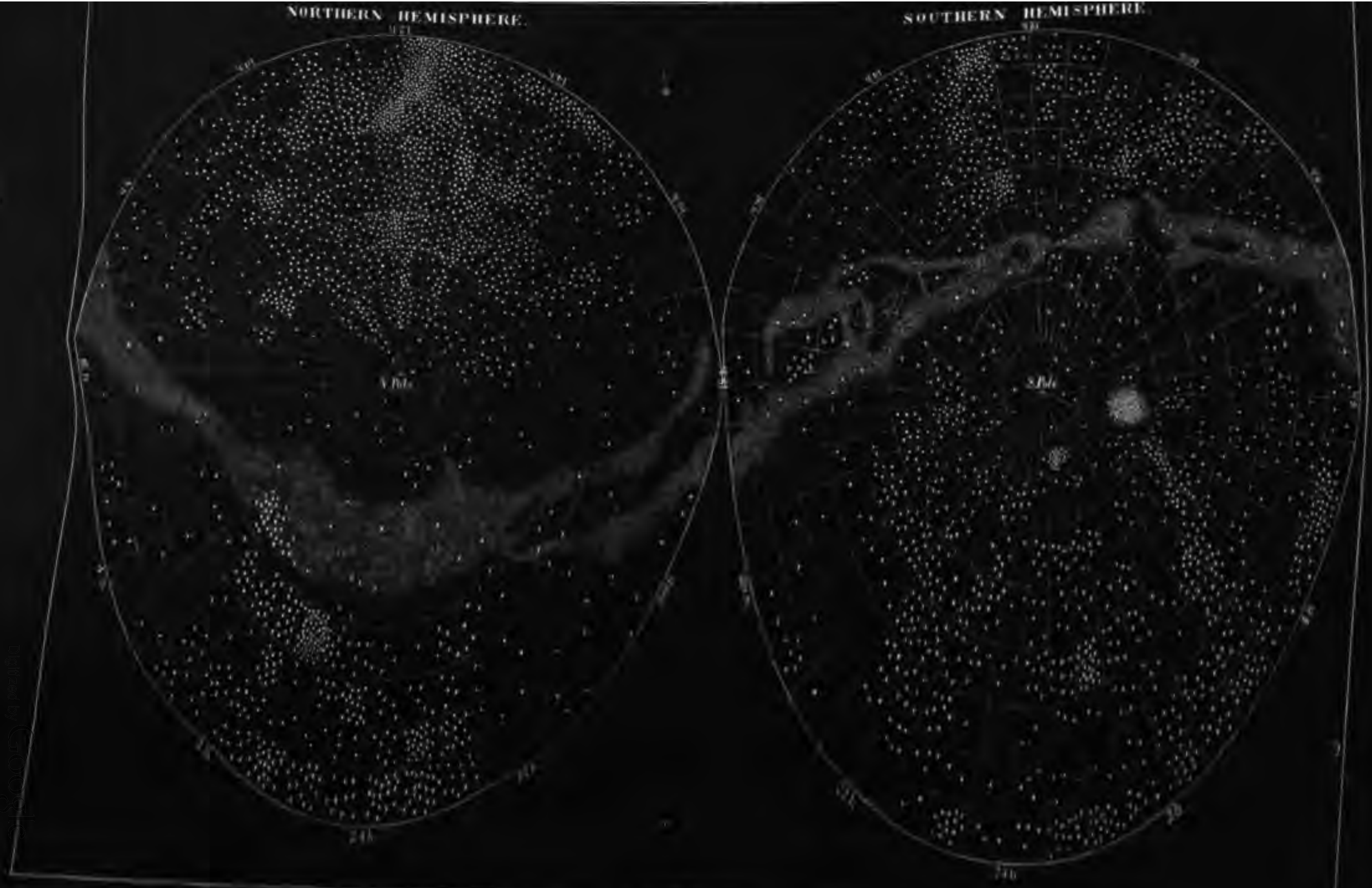
prevent anything like a special selection and combination of all the richest regions, for comparison with all the poorest regions taken together.

* The calculation of the exact chance would be laborious in the extreme. The above result corresponds to a calculation in which all the processes are simplified—but in such sort as to increase the chance. The real chance is undoubtedly far more minute. Yet the fraction actually resulting from this approximate calculation has, with unity for numerator, a denominator containing no less than 132 figures.

accepted theories. We have also been compelled to adopt the opinion that the milky light of the galaxy comes from minute stars, amidst clusters of which lie the lucid stars that are so densely strewn along the Milky Way. These conclusions alone, if accepted—and I can see no possible way of accounting otherwise for the observed relations—amount to a revolution in the science of sidereal astronomy. They not only oppose themselves completely to the accepted theories, but they point to altogether novel modes of dealing with the problems presented by the stellar hosts. The method of taking averages is discredited, and more cautious modes of inquiry—modes of piecemeal research, so to speak— are suggested as advisable. It may be worth while to notice that some time since I pointed to the importance of the conclusions which result from a careful analysis of the minute apparent motions of the fixed stars. Those conclusions correspond perfectly with the conclusions arrived at in the present paper. One mode of research confirms the other. But there are yet other processes of inquiry, by which the details of the sidereal system, the hitherto neglected *minutiæ* of the heavens, can be made to exhibit to us grand and as yet unthought of laws of stellar distribution. I commend such inquiries to the attention of those who have more leisure than I have to pursue them.

Fraser's Magazine for October 1870.



Isographic Projection.

I. DISTRIBUTION of the NEBULÆ.

R. A. Proctor.

DIS[illegible]

RATHER more [illegible] Abbe, [illegible] the [illegible] before the A[illegible] able contri[illegible] ment exhibiting the [illegible] are distributed o[illegible] I had sent to the [illegible] St[illegible] subject. Until this paper [illegible] distribution of matte[illegible] falls within our cognisance [illegible] liberty to bring before the Society [illegible] for a long time in my though[illegible] matter, and especially of [illegible] Student, the series of papers [illegible] This is, therefore, the earliest opp[illegible] mitting the views I speak of [illegible] Astronomical Society.

The three pairs of maps w[illegible] (Plates III., IV., and V.) have [illegible] able table prepared by Mr. Abbe; [illegible] not [illegible] that their teachings differ in [illegible] from those conveyed by maps [illegible] similar table prepared by Sir John [illegible] many years [illegible] It is important that this [illegible] attended to. [illegible] ([illegible]) Herschel's table was [illegible] 3812 nebulae, while Mr. Ab[illegible] of 5079 nebulae; (2) Hers[illegible] n con[illegible]

DISTRIBUTION OF THE NEBULÆ.

RATHER more than two years ago, a paper by Mr. Cleveland Abbe, on the distribution of the nebulæ in space, was read before the Astronomical Society. That paper included a valuable contribution to science, in the form of a tabular statement exhibiting the manner in which the irresolvable nebulæ are distributed over the celestial sphere. A few weeks before I had sent to the editor of the *Student* a paper on the same subject. Until this paper and four others dealing with the distribution of matter throughout that portion of space which falls within our cognisance had appeared, I did not feel at liberty to bring before the Society certain views which had been for a long time in my thoughts. Owing to the press of other matter, and especially of other astronomical matter in the *Student*, the series of papers was only completed in April 1869. This is, therefore, the earliest opportunity I have had of submitting the views I speak of to the judgment of the Royal Astronomical Society.

The three pairs of maps which accompany this paper (Plates III., IV., and V.) have been formed from the valuable table prepared by Mr. Abbe; but it is worthy of notice that their teachings differ in no important respect from those conveyed by maps which I formed from a similar table prepared by Sir John Herschel many years ago. It is important that this should be attended to, because (1) Herschel's table was formed from a catalogue of only 3812 nebulæ, while Mr. Abbe's was formed from a catalogue of 5079 nebulæ; (2) Herschel's tabulation corresponded to a division of the sphere into spaces of 1^h in R.A. by 15° in Decl., while Mr. Abbe's took spaces of only 30^m in R.A. and 10° in Decl.; and (3) Herschel included all nebular objects in his tabulation, while Mr. Abbe drew a distinction between the various orders of nebulæ, so that his table, as finally drawn up, included only 4053 irresolvable nebulæ. When we find that all the refinements thus introduced by Mr. Abbe, and an increase of nearly one-third in the total number of objects dealt with, produce no appreciable change in the indications which the nebulæ present of being aggregated together according to certain definite, and, as I think, very significant laws, we seem free to conclude that no extension of telescopic observation can appreciably affect our views respecting the distribution of the nebulæ. If the second tabulation had indicated laws appreciably different from those which the first had exhibited, the conclusion would have been that we must wait for an increase of knowledge before venturing to theorise; as it is, we may proceed to deal with the facts before us with some confidence as to the general accuracy of their indications. We see, then, that the two tables have an increased value when considered together.

I would first call attention to the pair of maps in Plate III. The plan of projection is the isographic projection described in my 'Handbook of the Stars.'* The meridians and parallels are so drawn as to correspond to Mr. Abbe's tabulation, and in each space as many nebulæ as his table indicates are marked in, so as to spread pretty uniformly over the space. Thus the projection being isographic, the maps represent the actual distribution of the nebulæ (as to density of aggregation, &c.) over the northern and southern hemispheres.

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* After that book was published I found that Sir J. Herschel had already suggested this construction. The coincidence is not very surprising when it is remembered that the problem of a central isographic projection is a very simple one, and has only one solution.

Now when this is done we see that there is much more in the arrangement of the nebulæ than that thinning off in the neighbourhood of the Milky Way which Mr. Abbe had recognised. No one can look at the general aspect of the two maps without seeing that the suggested distribution of the nebulæ in a figure roughly resembling a prolate spheroid, having the plane of the Milky Way at right angles to its longer axis, is wholly insufficient to account for the actual aspect of the maps. The northern map accords better with this view than the southern; but even in the former there is an irregularity in the clustering, an occasional evidence of streaminess, and in places a sharpness in the outline of the group which is altogether different from what would be seen if the nebulæ formed a figure even roughly resembling that suggested by Mr. Abbe. In the southern map all these features are exaggerated. In this map also a new and unexpected feature strikes us. We see that the Nubeculæ are associated by well-marked nebular streams with the main southern group.

But now it is obvious that the pair of maps in Plate III. requires to be supplemented by maps which shall exhibit—(1) the two groups of nebulæ in their entirety, and (2) the great zone bare of nebulæ which divides one group from the other.

The pair of maps in Plate IV. is intended to satisfy condition (1) as nearly as can be done on equatorial maps. The projection corresponds to that used in Plate III.* The first map of the pair exhibits the nebular group to the north of the Milky Way, the second exhibits the nebulæ to the south of that zone. It will be noticed that the contrast between the two groups is very striking.

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* So far as I am aware the pairs of maps forming Plates IV. and V. are the first in which the relations of the nebulæ have been presented isographically on any but a polar projection.

The northern group presents a somewhat stratified aspect, and we can understand how it was that Sir W. Herschel, observing only in northern latitudes, conceived that the nebulæ might be found to be arranged in a zone somewhat resembling the Milky Way, but nearly at right angles to it. On a closer inspection, however, we recognise in the northern cluster a decidedly streamy character.*

The southern group of nebulæ presents features of a very interesting character. As in the northern group, the general aspect of the system is streamy, but this character is much more marked in the southern group. A peculiarity is also noticeable in the fact that the most striking aggregations are at the *extremities* of the streams. In fact, the central part of the whole group, a region exactly opposite the great northern aggregation of nebulæ, is almost vacant. We have, again, distinct evidence of the connection between the Magellanic Clouds and the southern nebular group. The Nubeculæ are, in fact, merely instances, more marked than

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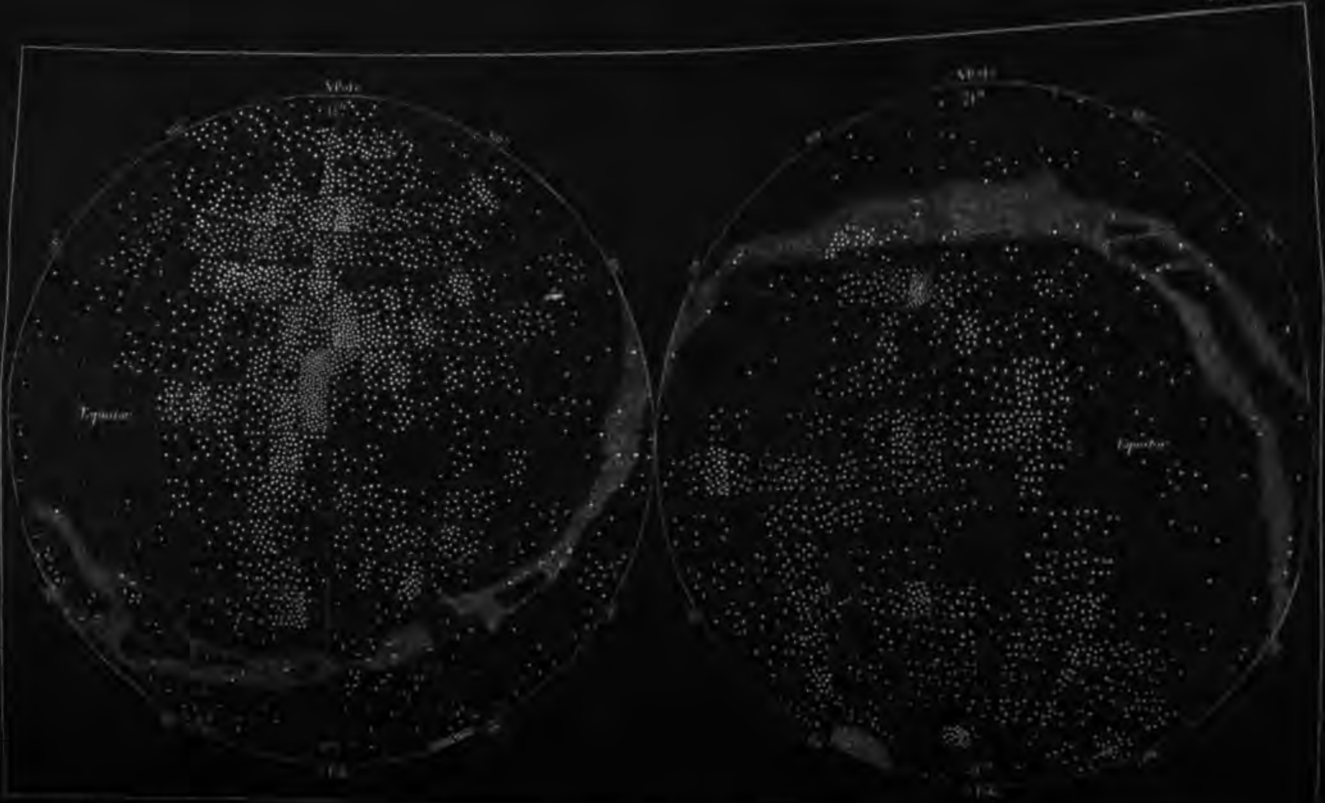
* It is to be noticed that it would be perfectly fair to strengthen the indications of these maps, by spreading the dots which represent the nebulæ less regularly over the spaces they belong to. At first sight this seems illegitimate. It might be argued, for example, that to take the streams presented to us on a first inspection, and to add to their obviousness by a special arrangement of the dots in the figure, is nothing less than to adopt an hypothesis, and to arrange our facts so as to seem to bear it out. But in reality this is not so. The process of arrangement so as to intensify the aggregations of dots and to make the vacuities more clearly apparent corresponds exactly to the process by which, having a series of ordinates in any graphic construction, we take a *curve* through their extremities instead of a series of rectangular cross-lines. The curved lines, in this case, slope upwards towards the direction of the longer ordinates, and downwards towards the direction of the shorter; and precisely in the same way the dots in any of the spaces in such maps as we are now dealing with should aggregate towards the direction in which there are greater aggregations, and be more sparsely strewn in the direction towards which there are vacuities. Had this been done the streaminess already clearly apparent in the arrangement of the nebulæ would have been rendered much more obvious. Such a plan, so far from being unfair, may be shown to be that which, according to the doctrine of chances, gives the arrangement which would most probably be observed if each nebula had been marked in in its proper place.

PLATE IV



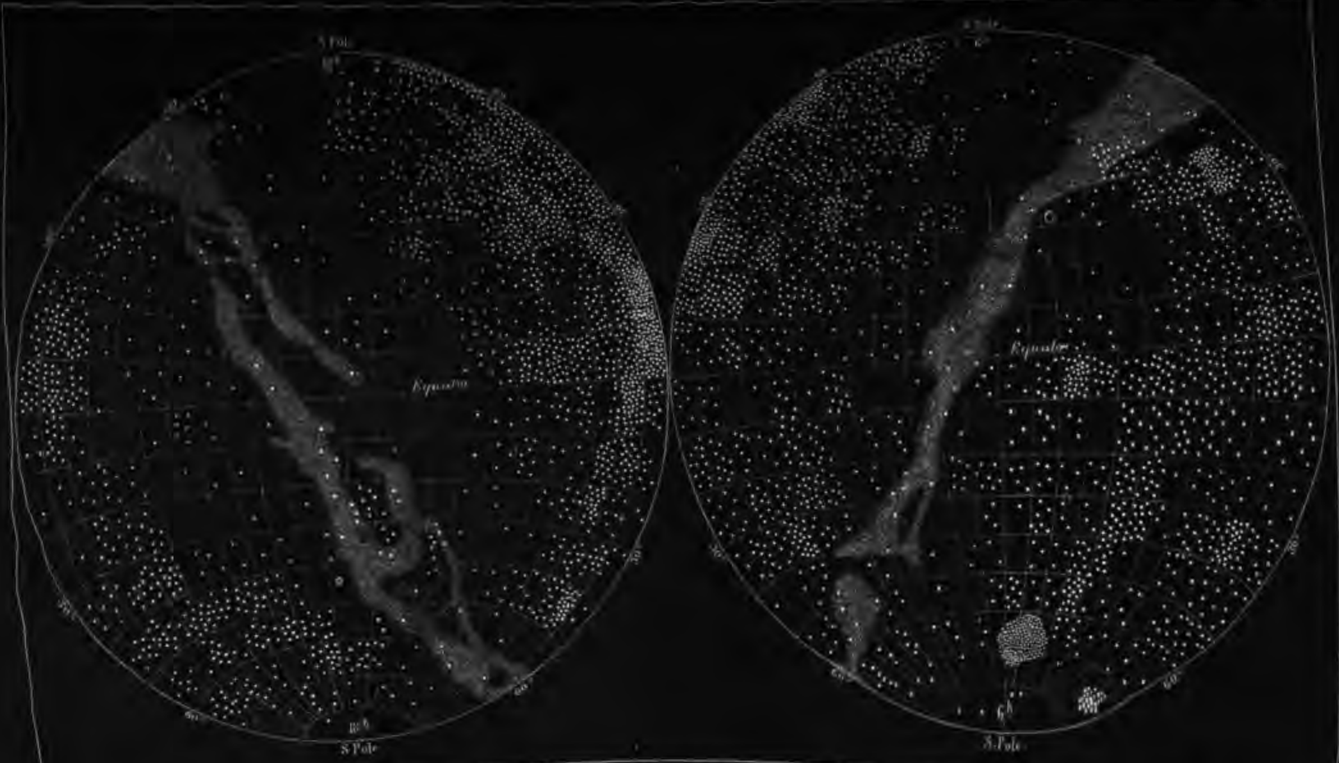
Stereographic Projection.

II. DISTRIBUTION OF THE NEBULÆ

[illegible]

R. A. Proctor

PLATE V.



Isographic Projection.

III. DISTRIBUTION OF THE NEBULÆ

The Zone of Few Nebulæ.

R. A. Proctor.

the others, of the tendency to aggregation at the extremities of the nebular streams which form the great southern group.

I now turn to the pair of maps in Plate V., which serve to bring out more especially the great vacant zone. This I hold to be the most significant of all the features presented by the nebular system. It will be observed that there is no mistaking the fact that a true zone of nebular dispersion (if I may use that term as the converse of aggregation) exists in the heavens. Nor can the general agreement of this zone with the galactic zone be called in question. I would now ask whether the conclusions to be drawn from these peculiarities ought not to be somewhat different from those which have been commonly accepted.

In the first place, I would remark that Sir W. Herschel was disposed to look on the fact that the zone of nebulæ which he supposed to exist in the heavens, lies at right angles, or nearly so, to the Milky Way, as not accidental. The peculiarity we actually have to deal with, however, is much more significant; first, because the agreement of two zones in position is a circumstance which is antecedently much more improbable than mere perpendicularity of intersection; and secondly, because the existence of a zone of dispersion is in itself a much more remarkable circumstance than the existence of a zone of aggregation. On the first point, it is merely necessary to remark that to secure perpendicularity between two zones it is requisite only that the poles of one zone should lie upon the other, which can happen in an infinite number of ways; whereas, to secure coincidence, the poles of one zone must coincide in position with the poles of the other, which can happen only in one way. On the second point, it may be noticed that the existence of a zone of dispersion would be very remarkable even if the zone were not particularly well marked; because all that we know of the universe leads us to look upon systems aggregated into the shape of cylinders, prolate spheroids, or the like, as much less likely to be met with than systems in the shape of discs, rings, oblate spheroids, or the like; and it is only figures of the former sort which can result in showing a zone of dispersion, figures of the latter kind exhibiting (as in the case of the sidereal system) a zone of aggregation. But a very well-marked zone of dispersion, as in the case of the nebular system, is a yet more significant phenomenon, because it can only be explained by looking upon the aggregations between which it lies as distinct systems. Looking on the two nebular clusters in this light, the fact that the Milky Way agrees exactly in position with the zone which separates them, becomes one which we are bound to account for in forming any theory of the nebulæ. In fact, when dealing with the aggregation of nebulæ near the north pole of the galactic system, Sir John Herschel expressed this very opinion; though he has nowhere indicated, so far as I am aware, the conclusion which seems to me to follow directly from that opinion:—

I cannot see how we can look upon the coincidence I have spoken of as *not* accidental, without being led to the conclusion that the nebular and stellar systems are parts of a single scheme. If the nebulæ were external star-galaxies, the coincidence might have happened, but it would be accidental. If they formed a system 'distinct from the sidereal system, though involving, and perhaps to a certain extent intermixed with the latter,' as Sir John Herschel suggested, the coincidence would still be accidental. If 'our system' (again to quote Sir J. Herschel) 'lies outside the denser part of the cluster, but is involved within one of its outlying members,' or, 'forms *an element* of some one of its protuberances or branches,' yet, again, the coincidence would be accidental. No theory which looks upon our galaxy as simply a member of the nebular system can possibly be reconciled with the view that the position of the zone of nebular dispersion is otherwise than accidental, unless we assume either (i.) that *all* the members of the system, or at least all in our neighbourhood, have a position parallel to that of our galaxy, or at right angles to the length of the nebular system, or (ii.) that there is some peculiarity in the galactic stratum preventing us from looking so far out into space along its length as in other directions. The first supposition is disposed of by the briefest study of the nebulæ, which (looked upon as disc-shaped universes) have every variety of position. The second has a greater appearance of probability, yet it would be easy to dispose of it on its own merits. It must be dismissed, however, for a reason associated with a matter which yet remains to be considered.

We have seen that the nebulæ belonging to the Magellanic Clouds are associated in a very obvious manner with the rest of the nebular system. But it must not be forgotten that these strange objects are also intimately associated with the sidereal system. In fact, their visibility to the naked eye is due entirely to the fact that they contain a number of small stars. Here, then, the sidereal and nebular systems are seen as they are nowhere else to be observed, *intermixed.* Perversely enough, this peculiarity, which seems so obviously to indicate an intimate association between the two systems, has been held by many distinguished astronomers to prove that the Nubeculæ belong neither to the sidereal nor to the nebular system. Sir John Herschel has distinctly pointed out the conclusions which should be drawn from the evidence given by the Nubeculæ; yet he nowhere definitely rejects views to which those conclusions seem directly opposed. He simply remarks that we may be led to look with some suspicion upon views which 'in former pages had been somewhat positively insisted upon.' *

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* I may remark here, in passing, that the tentative and philosophical tone in which Sir John Herschel reasoned respecting the universe, the light

The evidence afforded by the Nubeculæ seems to me decisive in favour of the intimate association between the stellar and nebular systems, which I have been endeavouring to establish. When it is combined with the evidence before adduced, no reasonable doubt can exist, I think, that the stars and nebulæ are members of the same scheme. There may be individual nebulæ which are true external universes —it is possible, for example, that the spiral nebulæ may be of this class, as also perhaps the Andromeda nebula, and a few others—but it seems demonstrated that for one nebula which is really external there are hundreds which are associated with the sidereal system.

What the processes may be which have led to the present arrangement of the great scheme of stars and nebulæ, I shall not venture even to conjecture. I may remark, however, that the great majority of the nebulæ constituting the northern and southern clusters may be looked upon as owing their present constitution to the fact that they are outside the region of most active stellar aggregation, and not to any difference in their original structure. I believe that the irresolvable and scarcely resolvable nebulæ are composed of stars really smaller and really closer than those forming the clusters and easily resolvable nebulæ. In favour of this view I would adduce a peculiarity exhibited in Mr. Cleveland

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grasp with which he held the theories he had himself developed, and the wide range over which his views extended, have, on the one hand, encouraged me to adopt views opposed to those which he sanctioned by his approval, and, on the other, to put forward views which he had already mentioned, but to which I have been led by independent considerations. His whole treatment of the subject of the sidereal and nebular systems indicated that the views he put forward were intended for the most part rather as suggestions than as theories. This power of weighing evidence without reference to preconceived opinions, of indicating the theories suggested by this or that portion of the evidence and as readily indicating reasons which must lead to the abandonment of such theories, may be looked upon as characteristic of the Herschel family. It would be well for the cause of scientific progress if the power were not so uncommon as it is.

Abbe's tabulation of the nebulæ, the true meaning of which seems to have escaped him.

If the objects in Sir J. Herschel's Catalogue are arranged in the following order,—(i.) clusters, (ii.) easily resolvable globular clusters, (iii.) resolvable globular clusters, (iv.) resolvable nebulæ, and (v.) irresolvable nebulæ, and the arrangement of these classes of objects with reference to a galactic zone 10° wide be considered, we find that objects in class (i.) are obviously aggregated on the galactic zone; objects in class (ii.) less so; those in class (iii.) still less so; those in class (iv.) are aggregated away from the galactic zone; and those in class (v.) are aggregated almost exclusively outside that zone. When we take a galactic zone 30° wide, we have—objects in class (i.) almost exclusively on the galactic zone; those in class (ii.) decidedly aggregated there; those in class (iii.) somewhat less so; while those in the other classes present much the same relations as before.

This plainly shows that the first three of these classes are associated with the galaxy in proportion to their resolvability; the fourth class, which is still resolvable, is removed from the neighbourhood of the galaxy; and the fifth still more so. Now, can we look upon the distinction between the resolvable nebulæ and the resolvable clusters as so marked that whereas we can admit the latter to be associated with the galaxy (or rather we are forced to do so), we must look upon the former as lying far away beyond the range of the sidereal system? But even if we must do so, how can we then account for the fact that the irresolvable nebulæ show a greater antipathy to the neighbourhood of the galaxy than their resolvable fellow-systems? If we do not, if we look on the resolvable nebulæ as belonging to the sidereal scheme, notwithstanding their aggregation away from the galactic zone, what reason can we assign for forming a different opinion of the irresolvable nebulæ which exhibit precisely the same relation, only in a more marked form? Finally, if we admit the irresolvable nebulæ into the galactic scheme, we cannot look upon greater distance as accounting for their irresolvability, because once they are recognised as associated with the galaxy, we can see no reason for the more distant members of the nebular part of the universe lying towards the poles of the galactic zone. The antecedent probability of such a relation prevailing that *distance* is the true cause of the irresolvability of the objects forming the great nebular clusters, is exceedingly minute. Of course, *cæteris paribus*, the more distant a cluster is, the less easily resolvable it will be. But we must look for some other cause than distance to account for the fact that nebulæ seen outside the Milky Way are less easily resolvable than nebulæ seen upon that zone. I believe that cause may be assumed not unreasonably to be the difference in the circumstances under which the galactic and the extra-galactic nebulæ have reached their present state.

I have hitherto omitted all reference to the gaseous nebulæ. So far as present observation goes, these objects show a decided preference for the neighbourhood of the galactic zone. The planetary nebulæ, all of which are probably gaseous, are so distributed that two-thirds are on the wider galactic zone referred to above, whose area is less than one-fourth that of the whole sphere. The irregular gaseous nebulæ are all on or close to the Milky Way, except one, which is in the greater Magellanic Cloud; another proof, if any were wanting, that the Nubeculæ are not external systems.

There appear to me to be also laws associating the nebular and sidereal systems together. Outside the Milky Way and its immediate neighbourhood the nebulæ seem to be associated with the lucid stars—in this way, that, where there are many leading brilliants, there nebulæ seem densest. There is not a single marked vacancy in either of the nebular groups where there is not at the same time a marked absence of bright stars. It is noteworthy also that, of the two most marked nebular streams, one is coincident with the stellar stream recognised by the ancients in their figuring of the constellation Eridanus (the prolongation southwards being recognised by modern astronomers in the constellations Hydrus and Reticulum); the other coincides with the stellar stream associated by the ancients with the water from the vase of Aquarius. This association between stellar and nebular aggregation seems to occupy a character midway between the yet closer relation observed in the Nubeculæ, and the directly contrary relation observed in the neighbourhood of the Milky Way.

Monthly Notices of the Royal Astronomical Society for October 1869.

A NEW THEORY OF THE MILKY WAY.

Sir W. Herschel's respect for existing analogies—a quality which is perhaps of all others the safest guide for the scientific explorer—led him to adopt as the means of interpreting his noble series of star-guagings the hypothesis that there is a general uniformity in the distribution of the stars through space. He adopted this hypothesis not from a conviction of its being actually true, nor even from the belief that it is approximately so, but simply because existing analogies seemed to render it probable, and because it formed a convenient basis for calculation. The existing analogies were those presented by the solar system. In this system, Sir W. Herschel recognised a number of discrete bodies, not equal indeed, but comparable *inter se* in magnitude; not uniformly distributed, but still not aggregated towards one or another part of the solar scheme. And making such modifications as seemed requisite in comparing a system not regulated by a vast central orb with a scheme like our solar system, it seemed likely to him that a general equality of magnitude, and a general uniformity of distribution, might be found to prevail among the members of the sidereal system.

We now know that the ideas which astronomers had formed of the solar system in Sir W. Herschel's day were very far indeed from being correct. We see in the solar system a complexity of detail, and a variety of form, structure, aggregation, and motion, which were altogether unknown a century ago. And I cannot doubt that if the view we have of the solar system had been presented to Sir W. Herschel, he would have adopted as the basis of his star-guagings an hypothesis differing altogether from that of which he actually availed himself. He would have argued that as in the solar system there are bodies like the planets far surpassing the other members of the scheme in magnitude and in importance, as it contains zones of minute bodies, such as the asteroids and the satellites composing the rings of Saturn, myriads of meteoric systems, and countless thousands of cometic systems, so doubtless in the sidereal system there are many forms of matter. If the analogy of the solar system is to be our guide, we must look for suns equalling or surpassing our own in magnitude and splendour; for clusters and systems of minor suns, whose united mass may fall short of the mass of one of the primary stars; for aggregations of matter in portions relatively so minute as not even to be comparable with the small stars found in true star-clusters; and, finally, for systems composed of materials, or at least of forms of matter, differing as widely from the substance of the suns as the matter composing a comet does from the substance of the Earth or of Jupiter.

But even independently of analogies such as these, his own series of observations led Sir W. Herschel to feel more and more doubt, as he proceeded, respecting the hypothesis which he had made the basis of his calculations. It is only necessary to compare the later papers he sent in to the Royal Society with the earlier ones, to find that views altogether inconsistent with his initial hypothesis were opening out before him. It was in those later papers that he spoke of star-groups in the Milky Way clustering towards opposite regions of the heavens; of stars arranging themselves into separate systems; and of the signs which the heavens present of the action of processes of aggregation, causing 'the gradual dissolution of the Milky Way.'

Sir John Herschel, also, in carrying out the system of star-guaging among the Southern stars, was led to notice many features which the hypothesis of a tolerably uniform distribution of stars could not satisfactorily explain.

Judged according to Sir W. Herschel's fundamental hypothesis, the sidereal system came to be regarded as forming a figure resembling that of a *cloven disc*, and the Milky Way was explained as being due simply to the greater extension of the system in the direction of the medial plane of this disc. Sir John Herschel, however, from his observations of the Southern heavens, was led to suspect that this theory was not strictly correct. He speaks in one place of certain evidence, according to which the Milky Way would come to be regarded as a flat ring seen edgewise. And in many places he speaks of the difficulty of understanding certain features according to the views usually accepted.

It seems to me that the evidence collected respecting the Milky Way is sufficient to lead us to quite another view of its structure than that to which Sir W. Herschel was led by an hypothesis founded on the incomplete theories which astronomers in his day had formed respecting the solar system.

Let us regard the matter altogether independently of preconceived opinions, and judge simply as the evidence may seem to teach us.

In the woodcut, the outer figure represents the Milky Way according to the drawings and description of Sir John Herschel. The mode in which it is delineated needs no explanation.

Now, in regarding this picture of the Milky Way, we are forced, I think, to the conclusion that neither the cloven-disc theory, nor the flat-ring theory, accounts satisfactorily even for the principal features of the Milky Way. For example, the great gap which crosses it in Argo, nearly in the widest part of the single branch, seems utterly inexplicable on either theory. There is no way of accounting for that gap if we are really supposed to view the Milky

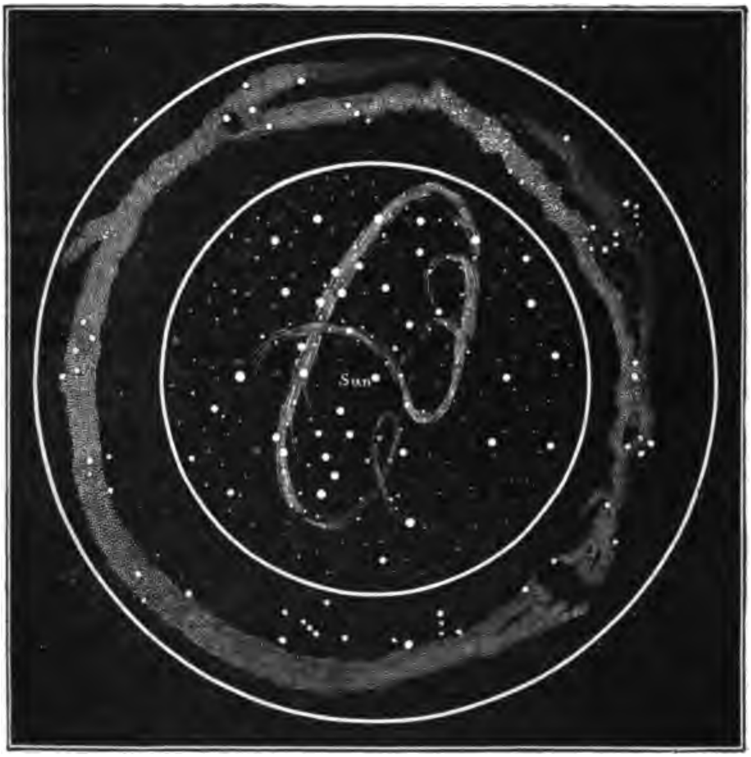

Fig. 18. Illustrating new theory of Milky Way.

Way from a point within its figure, and that figure resembles —however roughly—either a cloven disc or a flat ring.

But let us pass to other features. Travelling towards the right from the gap, we come to the strange semicircular cavity with a well-defined outline, which Sir John Herschel has described in such striking terms. A cavity of that figure is a remarkable phenomenon, and is surely inexplicable either on the flat-ring or cloven-disc theory. But the mere distinctness of the outline is one of the strongest possible proofs that the stars which form that portion of the Milky Way constitute a distinct clustering aggregation from which we are separated by an enormous and comparatively star-less interval.

We come next to the Great Coal-sack near Crux, almost opposite to which is a well-marked opening in Cygnus. There are also other strange openings through different parts of the Milky Way.

Now I cannot but think that an argument similar to that which Sir John Herschel has applied with so much force to the Magellanic Clouds applies to the openings in Crux and Cygnus. He argues that, because the Magellanic Clouds approach roughly to the circular figure, therefore, in all probability, their real figure is that of a sphere: the chance is small that one of them is a cylindrical system seen endwise; but the chance that both are is altogether evanescent. Now applying the same principle to the Coal-sacks, we are led with equal certainty to the conclusion that these apertures are not cylindrical or tunnel-shaped openings seen endwise, but *if they are really openings at all* they are openings through a system which is not very much deeper—measured in the direction of the line of sight—than the greatest width of the aperture itself.

Judged in this way, the parts of the Milky Way which lie round a 'Coal-sack' would have a roughly circular section, and not that enormous extension, in the direction of the line of light, which has been assigned to them.

I cannot see that this argument is at all less sound or less effective than that which has been applied by Sir John Herschel to the Magellanic Clouds.

There is another feature referred to, and I believe discovered, by Sir John Herschel, which is also full of meaning. I refer to the existence of narrow and sometimes convoluted streams of stars, branching out from the Milky Way itself. Sir John Herschel says of these that we ought to look on them as in all probability planes or scrolls of stars seen tangentially, and not as branch-shaped extensions bristling up from the general level of the Milky Way. And undoubtedly if the Milky Way really have a great extension in the direction of the line of sight, it is just that we should so regard these outlying streams. But if we judge of them without any reference at all to pre-existing theories, we are guided by strong arguments from probability to form a very different view. The chance that a plane system of stars, and still more a scroll of stars, should be turned so directly towards the Sun as to present to us the appearance of a straight or convoluted line or narrow stream of stars, is small indeed. The probability that several should be so situated may be regarded as evanescent.

Accepting these streams as having a roughly circular section, we are led to the conclusion that the Milky Way from which they extend has a similar section. In fact, as Sir John Herschel held these streams to be really planes or scrolls because (I assume) he assigned to the Milky Way a great lateral extension, so by inverting this argument I am led to believe that the Milky Way has not a great lateral extension (compared I mean with its thickness), because the streams extending from it have in all probability a section of roughly circular figure.

Other arguments there are that space will not permit me to dwell upon here, which point in the same direction.

Now, of course, if the Milky Way forms in reality a stream of stars *amidst the sidereal system*, the appearance which it presents upon the heavens might be expected to afford some information as to the shape of that stream, or at least of that portion which is cognisable by us. It must be admitted, however, that the problem of interpreting this wonderful stream is one of enormous difficulty. Perhaps it is one which man will never be able to accomplish in a perfectly satisfactory manner. It is only necessary to contemplate that marvellous maze of star-streams around Scorpio and its neighbourhood, and to read the account which Sir John Herschel gives (in his 'Cape Observations') of the telescopic aspect of this region, to feel how far we are at present from being able rightly to interpret the mysteries of the Galactic Circle.

The bolder and more striking features of that circle may, however, be studied with a better hope of their being successfully interpreted. A theory which will explain the gap in Argo, the wide break of one stream in Ophiuchus, the varying brightness of the principal stream in different parts of its length, and other features of this kind, may reasonably be sought for.

I have endeavoured in the inner circle of Fig. 18, p. 331, to indicate a spiral which seems to me to account for the most striking features of the Milky Way.

Following that spiral round from the part where the two loops approach each other, we have the following relations:

First of all, the gap is explained by the fact that the two loops do not meet. Then, remembering that the spiral is supposed not to lie in one plane, but (as the contorted figure of the Milky Way obviously suggests) to have been swayed out of that plane by varying attractive influences, we see that where the line of sight is directed tangentially to either loop, the Milky Way might be expected to have greater width than elsewhere. This explains the fan-shaped expansions on each side of the gap. Then on each side of these expansions we see the Milky Way double, which obviously corresponds to the relations exhibited by the two loops. Following the wider loop, we see that the double part of the Milky Way on this side extends nearly through a complete semi-circular arc. The Coal-sack is explicable as due to the apparent crossing of the two contorted streams which really are at different distances from the eye.* The break in the further branch seems readily explicable as due to the great distance of a portion of this branch. But here the theory derives a singular support from the actual relative brilliancy of different parts of the Milky Way in this neighbourhood. Every astronomer knows how strangely the light of the Milky Way varies in and near Cygnus. The branch which extends from the Northern Coal-sack towards Albireo is at first far the brightest, and then fades off so much that in Ophiuchus it is wholly lost. The other branch, on the contrary, gradually increases in brightness, until in Aquila, and, further on, in Sagittarius, it forms the brightest part of the whole Milky Way. Now this part, which is so very bright, corresponds to the part which my spiral brings so very near to the Sun.

Passing on to the termination of the second branch near Cygnus, it will be noticed how the spiral explains the strange extension of milky light from Cepheus towards the north pole.

Thence the stream is single, growing gradually fainter with increase of distance towards Canis Minor and Monoceros.

The spiral I have depicted seems so satisfactorily to account for several of the more striking features of the Milky Way as to suggest the idea that it probably corresponds somewhat closely to the real figure of that star-stream. I am sensible, however, that many peculiarities remain unexplained by, though they are by no means opposed to, my theory. It must be remembered that any objections founded on a presumed equality of stars throughout the Milky Way, or of a general uniformity of distribution throughout the spiral stream, do not require to be met; because at the very beginning of this inquiry I have abandoned such hypotheses as inconsistent with existing analogies.

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* In the large maps of the S. D. U. K. the Milky Way is depicted near Crux and Argo, as if the object of the draughtsman had been to support my theory. In Sir John Herschel's drawing, however, there are no such varieties of brilliancy.

For example, there may be parts of this Milky Way so constituted that, if we were to remove further and further from them, we should see them gradually assuming the form of irresolvable nebulosity. But there may be other parts which would never assume that appearance, let their distance be what it might—the distribution and magnitude of the component stars being such that the stars would vanish through effect of distance, before the distances apparently separating them became evanescent.

I may add as a striking confirmation of a portion of these views, that among the lucid stars along the part of the Milky Way which lies nearest to the Sun, according to my view, are those which have been actually found to be nearest to us.

It must be understood that I regard the Milky Way as simply the condensed part of a spiral of small stars, which has been swayed into its present figure by the influence of large stars—the lucid stars seen in the Milky Way. The myriads of small stars not lying in or near the Milky Way must yet belong to the same system, and in some instances seem to obey somewhat similar laws of aggregation. The nebulæ, so far as the evidence from probability extends, would appear to be groups formed from among those stars that have not fallen under the influence of the large stars which have brought the Milky Way spiral to its present figure. In the Magellanic Clouds, we see the action of processes which have tended to form spherical clusters of enormous dimensions, in which both forms of aggregation are met with.

Why, in different parts of the sidereal system different processes of aggregation should have taken place, we cannot yet distinctly see. But some of the striking discoveries which have recently been made by astronomers afford promise that light will soon be thrown on these perplexing questions.*

Monthly Notices of the Royal Astronomical Society for December 1870.

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* If my views respecting the Milky Way are correct, it obviously follows that there are parts of the Milky Way where traces of annual parallactic displacement might be looked for amongst *telescopic stars.* One instance of such motion would force us to modify all the views at present accepted respecting the sidereal system.

ON THE RESOLVABILITY OF STAR-GROUPS, REGARDED AS A TEST OF DISTANCE.

THERE are considerations connected with the resolvability of star-groups which have not hitherto received much attention, so far as I am aware. They bear somewhat importantly on the opinions we are to form respecting the distribution of matter throughout the sidereal system.

In the first place, the resolvability of such clustering aggregations of stars as obviously form part of the sidereal system has been held to be an important means of estimating the relative extension of different parts of that system. So long as a portion of such a clustering aggregation remains unresolved, it has been assumed that the limits of the system in that direction lie beyond the range of the telescope which thus fails to effect resolution, and therefore that the extension of the system in that direction is far greater than in other directions where the same telescope shows the stars projected discretely on a perfectly black background.

In the second place, in the case of definite groups of stars, which either lie beyond the limits of the sidereal system, or if within those limits are yet separated from other parts of the system, and surrounded on all sides by relatively barren regions, it has been commonly assumed that we have, in the telescopic powers necessary to effect resolution, a means of forming a general estimate of the distances at which such groups may lie.

It is my purpose here to indicate certain considerations which point to opposite conclusions as respects both these cases.

If we were to accept the conclusion that where a portion of the galaxy is not resolvable with powerful telescopic aid, the sidereal system has a relatively great extension, it would follow from the smallness of the areas which many of these portions of the galaxy present, that there is an extension of the system in those directions into long spike-shaped projections, lying in a direction pointing exactly towards the solar system. When Sir William Herschel, for example, speaks of a region of this sort, of limited extent, which his great 40-feet reflector was unable to resolve, we must accept the conclusion that there is one of these spike-shaped projections extending (according to Sir William Herschel's own estimate) no less than 2300 times further into space than a line drawn from the Sun to Sirius. It is not only contrary to every law of probability that this is the real state of the case; but even if we could suppose that in this and in other similar instances such spike-shaped projections could *by mere accident* be directed along lines extending radially from the Sun (that is, if we could get over the argument from probability), there would still remain mechanical objections to our believing in such an arrangement. Knowing as we do that all the stars are in motion under the influence of their mutual attractions, and apparently also under the influence of some other and far greater forces adequate to generate the enormous observed motions, we ought scarcely to be willing to recognise in any part of the system a law of distribution which could not result from any conceivable dynamical processes.

It seems more reasonable to conclude that, where a cluster presents the peculiarity considered, there is not enormous longitudinal extension, but a real peculiarity of constitution; that, in fact, the observer has not been penetrating further and further into space as he increased his telescopic power, but simply analysing more and more searchingly a definite region of space.

In fact, Sir William Herschel, in one of his later papers, was led to consider this as perhaps the true explanation of the matter; for in 1817 we find him saying that his star-guagings have in reality more direct reference to the condensation than to the distance of the stars, so that a greater number of stars in the field of view may be explained as well by a greater condensation of that portion of the galaxy, as by a greater extension of its figure in that direction in which the stars appear most numerous.*

Now, as regards the case of a distinct cluster of stars, let us consider first the effect of distance on a group of stars all equal in magnitude and separated from each other by equal intervals. Supposing such a cluster so placed that the naked eye could recognise each separate orb, and then to sweep rapidly away into space, would it become nebulous or not before vanishing from view? As the group passed away each separate orb would grow less and less bright, and the distances separating orb from orb would grow apparently smaller and smaller. And clearly if these distances became too small to be distinguished, while the stars of the group yet continued visible, there would result a nebulosity of appearance. But suppose that, on the contrary, the stars of the group became invisible when the group was at such a distance that the intervals separating star from star would not be indistinguishable (if only the stars were brighter). Then clearly the group would vanish with increasing distance without ever becoming nebulous. Clearly also, if a telescope were employed to bring the retreating group into view, the same conclusions would hold good. A group which would become nebulous to the naked eye before vanishing would become so when examined under a telescope, let the telescope's power be what it might; while a group which would vanish without becoming nebulous to the naked eye would not become nebulous before vanishing under telescopic vision, whatever the telescopic power employed.

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* It is rather surprising that in nearly all our treatises on astronomy the earlier papers by Sir William Herschel receive far more attention than those he wrote when at the zenith of his fame. There is only one work I know of (Professor Grant's noble *History of Physical Astronomy*), in which Sir W. Herschel's later labours are adequately represented.

It is clear, then, that the nebulosity of a star-group, whose members are equal and equally distributed, is a question not of distance merely, but of constitution—of the relation between the size and brightness of the constituent orbs and the distances which separate them from each other.

But we may extend such considerations to the case of star-groups containing orbs of different orders of magnitude. Supposing a group of this kind to be passing away into space—as in the former case—the question whether it would become nebulous at any stage or stages of its progress would depend on the question whether or not the order of stars about to disappear individually were congregated so closely that the eye could not distinguish the distances separating them. Clearly also it might be possible that an order of stars *not* about to disappear might present a nebulous appearance, in which case obviously all lower orders still remaining visible would be involved in that nebulous light. Such a cluster, in passing away from the eye, might also be nebulous at a certain distance, and become non-nebulous at a greater distance; all that would be necessary for such a result being that, while some of the lower orders of stars were distributed richly enough to present a nebulous appearance before vanishing, some of the higher orders should be so sparsely distributed as not to present a nebulous appearance before vanishing, or at any rate for some time after the lower orders *had* vanished.

It is further obvious that the same would be true if the retreating group were watched with a telescope of any power whatever (setting aside all question of the extinction of light in passing through space). The same appearances would be presented in precisely the same order when the group passed (star-order by star-order) out of the range of view of any telescope as when it passed out of the range of the unaided vision.

It follows that, if we apply telescopic power to a given group of stars, we can by no means conclude from the nebulosity of the group under such and such a power that the group lies at such and such a distance, unless we are prepared to believe in the existence of certain laws of constitution to which all stellar clusters are subject. But such a belief is not likely to find acceptance with those who are acquainted with the observed variety in the constitution of star-groups.

It happens also that we have direct evidence that irresolvable nebulosity affords no proof of relatively enormous distance. When Sir John Herschel was surveying the neighbourhood of the lesser Magellanic Cloud he found that near the edge the Nubecula Minor was irresolvable with the 18-inch reflector, whereas the heart of this Nubecula could be clearly resolved.* Now it needs no proof that, if the Nubecula Minor (setting aside the nebulæ existing within

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* I quote the following passages from Sir John Herschel's *Notes on the Nubecula Minor*. They are all I can find which bear on the question of resolvability.

'The edge of the "smaller cloud" comes on as a mere nebula.'

'In the edge of the cloud vision bad, &c. . . . the cloud is not resolved, and seems a very mysterious object.'

'We are now in *the cloud*. The field begins to be full of a faint light perfectly irresolvable.'

'I should consider about this place the body of *the cloud*, which is here fairly resolved into excessively minute stars, which, however, are certainly seen with the left eye.'

'Re-examined by the side motion the whole cloud, in general and in detail. The main body *is* resolved, but barely. I see the stars with the left eye. It is not like the *stippled* ground of the sky. The borders fade away quite insenit) were constituted of stars according to the generally uniform laws assigned to the constitution of the sidereal system, the centre of the Nubecula would be the part whose resolution would be most difficult. It is evident, therefore, that the outer parts of the Nubecula are constituted differently from the central region, and the possibility is suggested that the smaller stars seen in the central region belong in reality to the outer shell, whose real character is indicated by the irresolvability of the outer parts of the Nubecula's *disc* (as distinguished from the Nubecula's substance).

In this instance, then, it is distinctly proved that the irresolvability of a celestial region under Sir John Herschel's 20-feet reflector is no proof of relatively enormous distance. But what is thus proved for a certain telescopic power must be true of all telescopic powers. Hence, whatever the power may be under which a certain region appears nebulous, we have no proof that the stars contained within that region are further off than stars within a region resolvable under that power. But since this must be true of all powers, it must be true of naked-eye vision. Hence the stars forming the galaxy are not necessarily further off than those star-groups which the eye can resolve.

One important conclusion which is, I think, fairly deducible from what has been shown, is that, supposing a spiral of small stars such as I have suggested that the Milky Way may be, should extend, along a part of its length, so far from the eye as to become invisible through distance, we ought not to expect that in passing from the visible part to this invisible portion all orders of resolvability down to utter

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sibly, and are less or not at all resolved. The body of the cloud does not congregate *much* into knots, and altogether it is in no way a striking object apart from the nebula and clusters.'

' Upper limit, *but here it is starry, at the other limit nebulous.*'

irresolvability in the most powerful telescopes ought to be recognised. On the contrary, this part of the spiral might exhibit in succession all the orders of change which the retreating group considered above was shown to be capable of exhibiting (on a certain, not improbable, assumption as to its structure).

But my object is not so much to find evidence in favour of a special theory about a certain portion of the sidereal system as to indicate the varieties of appearance which are to be looked for in different parts of that system—varieties which are, in fact, as likely to be met with (according to my views of the nature of that system) around the poles of the galaxy as in the richest portions of that wonderfully complex zone.

Monthly Notices of the Royal Astronomical Society for May 1870.

A PROPOSAL FOR A SERIES OF SYSTEMATIC SURVEYS OF THE STAR-DEPTHS.

When we consider the vast addition to our knowledge, and the yet vaster widening of our conceptions respecting the star-depths, which resulted from the labours of Sir William Herschel and the great man whose death science has lately had to deplore, we cannot doubt that more complete surveys, if such could be carried out, would well repay the pains bestowed upon them. I apprehend that not the least among the purposes which the elder Herschel proposed to fulfil when he commenced that first great survey, was to show astronomers how much the survey of the heavens was needed; and I imagine that he would have been the last to approve of that supineness (arising, perhaps, from an exaggerated respect for his labours) which has prevented all save his son from pursuing the path which he was the first to indicate. As to the opinion of Sir John Herschel on this matter, it is unnecessary that I should speak; because he has in many parts of his works urged in his own earnest manner how desirable it is that the celestial depths should be studied much more closely than they could be by only two observers, however skilful and energetic, or however patiently they continued their labours for many successive years.

I believe that what is now specially required is a series of systematic surveys, proceeding on a principle quite different from that on which the Herschels found it necessary to pursue *their* researches. A first survey was very properly, or, I should say, it was necessarily applied with reference rather to the average distribution of the stars than to the special laws of distribution which may be found to prevail either when we extend our survey over the different parts of the celestial sphere, or when we vary the range of our vision by employing different telescopic powers.

I venture now to impress most earnestly on those who have sufficient leisure and possess the necessary instrumental means for carrying out systematic observation, the extreme importance of surveys of the star-depths on methods devised with reference to both these relations.

Let me briefly indicate what is at present wanted, noting at the same time that the proposed observations should only be regarded as first steps in a progressive series of researches directed to the solution of the noblest of all the problems astronomers can deal with—the determination of the laws (so far as they are discoverable) according to which the heavens are constituted.

(1.) I have already mapped down isographically the stars visible to the unaided eye; and I think that no one can study my isographic chart, or the numerical statistics which accompany it in the second edition of my 'Other Worlds,' without feeling that even what the heavens disclose to the unaided eye has a significance which has too long been suffered to escape recognition.

(2.) I am engaged * in mapping down isographically the stars in Argelander's charts of the northern heavens—324,198 stars in all, about 310,000 north of the equator, and about 14,000 within two degrees south of the equator. When this chart is completed, it will serve to show, so far as the northern heavens are concerned, what are the laws of distri-

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* This chart is now completed; and photographs of it, with photolithographed keymaps are published by Mr. Brothers, of St. Ann's Square, Manchester.

bution among those stars which lie within the range of a telescope 2½ inches in aperture. The correspondence between these laws in certain parts of the heavens, with those observed in the distribution of naked-eye stars on the one hand, or of the Milky Way, nebulæ, &c., on the other, will throw light on many questions of interest—for instance, on the numerical relations of large and small stars (stars really large and small, that is), and consequently on the evidence as to the distances of stars of different apparent orders. Light will also be thrown on many other questions of importance. That this is so I shall be able (I already know from the progress of the work) to establish; but at present it will suffice to notice, as evidence of the importance of such researches, that Struve, taking only one-tenth the number of stars in Argelander's charts, and dealing with those stars only by averages,* not only as respects extension in right ascension and declination, but also as respects probable extension in space, was yet led to results which are of extreme interest if admitted, and highly suggestive even if regarded as still open to question.

The system of charting with a telescope having an aperture of 2½-in. requires extension over the whole southern hemisphere. The results of this extension alone would be (I venture to predict) of the utmost possible interest. For the purpose of increasing our knowledge of the constitution of the star-depths, actual charting would not be needed, but only such a process of statistical enumeration as I propose in the case of larger apertures.

(3.) The Council of the Astronomical Society have kindly placed at my disposal the Sheepshanks Equatorial No. 3, having an aperture of 4⅝ inches, and I propose to apply this instrument to the enumeration of stars lying within the increased range corresponding to its aperture. The plan I propose to follow is to count the number of stars seen in equal fields taken all over each region surveyed, and then to map down the result isographically. It is probable that the very limited amount of leisure time I can devote to such researches may prevent me from surveying more than a small region of the northern heavens. But as my special object is to show what lessons such surveys are calculated to teach, I shall be well satisfied if I can thus, by example more effectively than by precept, engage others who have more leisure and instruments of about the size named to extend this survey to much larger regions. If the results are not charted by the observer himself, he should keep a guage-book from which an isographic chart could be formed hereafter.

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* How rough these were (intentionally, of course) is shown by the fact that the charted results did not even indicate the division of the Milky Way into two parts over a region crossed by Struve's section. 'C'est une suite naturelle,' says Struve on this point, 'de ce que notre recherche du disque ne s'est point faite dans tous les détails, mais *par heures entières* d'ascension droite.'

The southern heavens also should be surveyed with a telescope of about 4½ inches in aperture.

(4.) A survey on a similar plan should be carried out over the whole heavens with a telescope 9 inches in aperture.*

(5.) Star-guaging with powers about equal to those of the telescopes with which the Herschels surveyed the heavens, should be carried out on a much more complete plan than was possible in the case of astronomers engaged like the Herschels on many different branches of research simultaneously. In the present position of sidereal astronomy, more is to be gained by the complete survey even of a small region in this way (followed by careful isographical charting) than by an incomplete survey depending on a law of averages which has been proved *not* to prevail in stellar distribution.

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* I speak here with reference to refractors. If reflectors are used in such a survey a careful comparison of their light-gathering power, and that of refractors employed in the same sort of work, should be instituted beforehand. If this power were brought to an equality, or nearly so, different parts of the heavens might be simultaneously surveyed by observers using reflectors or refractors.

(6.) A good 4-ft. speculum should be applied to the same purpose, even though half-a-dozen generations might have to be occupied in the work.

Feeling absolutely certain of the extreme interest and importance of the results which would follow from such surveys as I have here advocated, I am by no means deterred by the largeness of the labours involved, even in the complete survey of the heavens with small apertures, from urging those who can do so to join actively in a work so valuable. There are so many amateur astronomers who have fine telescopes for which they can find no employment, and so many others who *do* find employment for their telescopes, but after a fashion tending in no sort to advance our knowledge, that the work ought not to languish for want of recruits. And I should imagine that no training, even, would be wanted by a large number of these recruits; because the eagerness with which telescopists are trying to divide difficult double stars, or to see planetary features which their telescopes are just *not* able to show, and so on, seems indicative of an earnest desire to acquire a fitness for useful work. I have named certain apertures which should at once be applied to the survey, but reflectors and refractors of five, six, seven, or any number of inches in aperture, could be most usefully employed in sidereal survey.

Monthly Notices of the Royal Astronomical Society for June 1871.

APPENDICES.

APPENDIX A.

A NEW DETERMINATION OF THE DIURNAL ROTATION OF THE PLANET MARS.

THE determination of a planet's period of rotation, to any great degree of exactness, may appear a waste of labour. Sir W. Herschel considered such problems worthy of attention, however, as supplying us with a time-measurer *external to the Earth*, and therefore possibly enabling us to detect slow changes in the Earth's rotation. The planet Mars, undisturbed by the attractions of a satellite (unless, indeed, some small moon as yet undiscovered attends upon him), and marked by easily recognisable features, is clearly the one to be selected for such a purpose; and although modern astronomers would hardly look to Mars for information respecting such changes, yet, I think, the bare possibility that at some future date the rotation of Mars may serve this purpose will make the investigation of the subject interesting to astronomers.

Having by me a large number of pictures of Mars, and especially a series of charming tracings kindly given to me by Mr. Dawes, I determined to test the periods assigned by Mädler and Kaiser ($24^h\ 37^m\ 23{\cdot}8^s$ and $24^h\ 37^m\ 22{\cdot}6^s$ respectively). I found that, when the pictures taken by Mr. Dawes in 1852, 1856, 1860, 1862, and 1864, were examined in pairs, the period $24^h\ 37^m\ 23^s$ was perfectly satisfactory. Satisfied, therefore, that this period (or 88,643 seconds) was undoubtedly very near the true one, I took longer intervals. After being misled for a time by fig. 3, plate xvii., in Lardner's 'Handbook of Astronomy,' which seemed to represent the same phase as Dawes' picture of Oct. 3, 1862 (see Lockyer's also of the same date, in vol. xxxii. of our 'Memoirs'), I noticed that, with the appended date and hour, that picture (so viewed) was irreconcileable with the picture of Sept. 14, 1830. Selecting the latter picture as far the more marked of the two, I arrived at the conclusion that the period of 88,643 seconds was slightly too great. Kaiser's value, 88,642·6, seemed, on the other hand, slightly too small. I proceeded to yet longer intervals with the provisional period 88,642·8, which I was satisfied was very nearly correct. The remainder of the calculation I will give more at length. So far as I am aware the rotation of Mars has never before been tested by such long intervals.

I took, first, Herschel's observation of Sept. 30, 1783, for comparison with Dawes' observation of Oct. 3, 1862. There are several peculiarities about this selection. First, the planet was nearly at the same part of its path (it is noteworthy that 42 revolutions of Mars differ by little more than two days from 79 revolutions of the Earth); secondly, he presented nearly the same face, and, thirdly, the dates of observation nearly coincide. It will be found that the interval actually separating the two observations (Herschel's made at $10^h 30^m$, Dawes at $12^h 15^m$) is

2,493,251,100 seconds.

(It will be remembered that 1800 was not a leap-year.) But there is a correction for phase, since the planet had not quite reached the phase seen by Herschel when Dawes observed it. There is also a slight correction for difference of geocentric longitude, about 4°. Both corrections are additive, and they amount together to about 4,000 seconds. The resulting period

2,493,255,100 seconds,

divided by 88,642·8, gives a quotient 28,127 *very nearly*. This is the number of revolutions which had elapsed between the two observations. Dividing, therefore, 2,493,255,100 seconds by 28,127, we get the period

88,642·76 seconds.

To test this we can apply a much more extended interval. For in the 'Phil. Transactions,' vol. i. 1666, there are two pictures taken by Hooke in 1666. They are dated March 3, 1665 ($0^h 20^m$ and $0^h 30^m$ morning), which being translated into our present style becomes March 13, 1666 ($12^m 20^h$ and $12^m 30^h$). These pictures, the only fairly recognisable ones in the set of pictures by Hooke and Cassini (shown in the same plate), represent that long sea running north and south with which astronomers are familiar.

I selected, first, a picture taken by Dawes, at $10^h 50^m$, April 24, 1856, in which year opposition occurred on April 2. Next I took Hooke's first observation for comparison with that made by Dawes at $11^h\ 46^m$, Nov. 26, 1864, an interval of more than 198 years; and, lastly, a series of clearly marked views of Mars, taken by Mr. Browning in January and February 1867, afforded me the opportunity of estimating the rotation of Mars by means of an interval of nearly 201 years.

I now present in a table the results of the calculation of these three long periods, viz. from March 12, 1666, $12^h\ 20^m$ (astronomical time and New Style) to

April 24th, 1856 . . . . . (i)
Nov. 26th, 1864 . . . . . (ii)
Feb. 23rd, 1867 . . . . . (iii)

Int.	Interval in seconds.	Cor. for Geoc. Long.	Cor. for Phase.	Corrected Int. in seconds.	No. of Rotations	Resulting Rotation Period.
(i)	5,999,524,200	0°	−12°	5,999,521,246	67,682	88,642·737
(ii)	6,270,650,760	−248	0	6,270,589,696	70,740	88,642·734
(iii)	6,341,394,300	−273	+ 3	6,341,326,590	71,538	88,642·734

Here the results have been brought into close agreement by selecting suitable values for the phase-corrections. That these values are not far from the truth will be seen by the copies of the views in question (see Plate II.); the arc from cross-line to cross-line along the equator of Mars being, of course, 30°.

The corrections for geocentric longitude may be depended upon as being within 1° of the truth. In Plate II. the positions of Mars and the Earth at the several epochs are indicated and connected by straight lines.* In determining the approximate position of the Earth at any date several circumstances have to be considered. Thus take the opposition of Mars in March 1666. I find that this took place at midnight (nearly) on March 18. But it will be seen that the opposition line is placed about one quarter of a degree in advance of the position given to the Earth on March 21. The explanation of this will illustrate the process applied to each epoch:—

My chart of the orbits of the four inner planets shows the position of the Earth from day to day, on the supposition that at noon on December 21 the Earth is at the winter solstice. Now, nearly enough for our purpose, this was the case in leap-year in the beginning of the seventeenth century. But the Earth was about 45' in advance of the winter solstice at noon on December 21 in leap-years *at the end* of the seventeenth century—therefore, in 1664, about 30′ in advance. Again, in each common year the Earth loses 15′, so that in 1666 there was at any hour of any day, after February 28, a loss of about 30′ from the position held by the Earth at the same day and hour in 1664. Therefore the position of the Earth at midnight, March 18, 1666, was about that which would correspond to that date as the chart stands; that is, about two days and a half's motion (or nearly 2½°) behind the point marked March 21. But owing to the precession of the equinoxes the lines ♋ ♑ and ♎ ♈ in my chart must be shifted about 2¾° forwards around their point of intersection to indicate the position they had in 1666. The difference ¼° indicates the amount by which the opposition-line in question is in advance of the position given to the Earth on March 21 in my chart.

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* The method of interpreting the lines and drawings in Plate II. will be better understood by following the full description of the final calculation in the following portion of this paper.

The close agreement between the results obtained—and that without any noteworthy *forcing* of the correction for phase, *the only doubtful point in the whole question*, induces me to look on the value

$$88{,}642{\cdot}735 \text{ sec. or } 24^h\ 37^m\ 22{\cdot}735^s$$

as very near the true value of Mars's rotation-period.

Mr. Browning having mentioned to me several months ago, that he hoped to be able to obtain several drawings of Mars at the present opposition, by means of his 12-inch reflector, it occurred to me that I might avail myself of so favourable an opportunity to test the estimate I had formed of the rotation-period of Mars. It is true that, when an estimate is formed from the rotations occurring in the course of so long a period as 201 years, one cannot expect any important correction to be obtained by throwing in the rotations of another biennial period. But there was this to be considered:—The drawings of Mars which I had used in the former calculations had not been constructed with any reference to a computation of this sort. Those who are familiar with the difficulties inherent in the observation of Mars will recognise the fact that the draughtsman might very well place a feature slightly on one side or the other of its true position. I have found this to be the case in drawings by the best observers, at least if the hour and minute appended is to be considered the true epoch at which the drawing is made. On the other hand, a simple observation having reference only to the central passage of some distinct marking is a matter about which there is much less chance of mistake. The experiments of Sir William Herschel as to the views which even unpractised observers form respecting the centering of a disc show how little chance of error there is in a matter of this sort; and in the case of a practised observer like Mr. Browning the chance of error was much diminished.

The special work, then, which I asked him to favour me by carrying out was the observation of a central passage of the Kaiser Sea (using the nomenclature of my chart of Mars)—that is, the long funnel-shaped sea running north and south, sometimes called the Hour-glass Sea. This feature is depicted in two views of Mars taken by Hooke on March 12, 1666 (New Style), at $12^h\ 20^m$ and $12^h\ 30^m$; and my former estimates of Mars' rotation-period had been obtained by comparing Hooke's views with drawings of the same feature by Herschel, Beer and Mädler, Dawes and Browning. From the close agreement between the two drawings taken by Hooke and the true figure given to the Kaiser Sea, I am disposed to place much more reliance on his observations than one would ordinarily accord to the work of the astronomers of the seventeenth century. There can be no doubt that he saw the Kaiser Sea under peculiarly favourable circumstances on that spring night in 1666, since his drawings will bear favourable comparison with any that have been obtained in recent times with instruments of moderate power.

So much for the observation which limits one end of the long period.

The observation next to be considered is the one which Mr. Browning obligingly made at my request. According to this observation the Kaiser Sea was centrally situated on the disc of Mars on February 4, at $11^h\ 3^m$ G.M.T. A few minutes before this hour Mr. Browning felt certain that the sea was to the left of the centre, and a few minutes after he felt certain it was to the right. This will be understood when we remember that a point near the middle of the disc traverses $\frac{1}{470}$th of the disc's diameter in a minute. So that if we take a range of eight minutes on either side of the epoch of central passage, we get a range of motion equivalent to about $\frac{1}{59}$th part of the disc's diameter, and there would be no mistaking so considerable a motion as this.

At the hour of observation the longitude of the Earth was about 135¼ degrees; that of Mars about 141¼ degrees. Marking down the points thus indicated upon my chart of the orbits of the four inner planets, and drawing in also the points occupied by the two planets at the epoch of Hooke's observation, I found the difference of Mars' geocentric longitude to be about 35½ degrees. From the care taken in the construction I think this result may be trusted as within a quarter of a degree at the outside. It results that the correction for geocentric longitude may be obtained by adding the period in which the planet rotates through an angle of 35½ degrees, or by subtracting the period of rotation through 324½ degrees.

Next as to the correction for phase. It is obvious from Hooke's first drawing—see 'Monthly Notices' for January 1868—that the Kaiser Sea was some 18 degrees (or perhaps rather more) from central passage at the hour of observation. Thus we must deduct a further interval equivalent to the period of the planet's rotation through somewhat more than 18 degrees, because Browning observed the planet when rotated so many degrees further than when seen by Hooke.

We have then, in all, a deduction equal to the period of the planet's rotation through $324\frac{1}{2}° + 18\frac{1}{2}°$ (say) $= 343°$. This corresponds to a deduction of

$$\frac{343}{360} \times 88{,}643 \text{ seconds},$$

or

$$84{,}457 \text{ seconds}.$$

Now the total number of seconds between

$$\text{March 12th, 1666,} \quad 12^{h}\ 20^{m},$$

and

$$\text{February 4th, 1869,} \quad 11^{h}\ 3^{m},$$

will be found to be

$$6{,}402{,}926{,}580 \text{ seconds},$$

and the corrected interval is therefore,

$$(6{,}402{,}926{,}580 - 84{,}457) \text{ seconds}$$
$$= 6{,}402{,}842{,}123 \text{ seconds}.$$

The number of rotations obtained by dividing this period by a rough estimate of the rotation-period at 88,642·7 seconds is found to be 72,232.

Lastly, dividing 6,402,842,123 by 72,232, we obtain for the rotation-period the value

$$88{,}642{\cdot}736 \text{ seconds},$$

or

$$22^{h}\ 37^{m}\ 22{\cdot}736 \text{ seconds},$$

the value named in my last paper on the subject being

$22^{h}\ 37^{m}\ 22{\cdot}735$ seconds,

and the limits of probable error

$0{\cdot}005$ seconds.

The three intervals before examined, ranging from 190 to 201 years, gave severally

88,642·737 seconds
88,642·734 „
88,642·734 „

The interval now considered, one of nearly 203 years, gives a value between the greatest of these and the two others; and as all the values now obtained lie between

88,642·73 s. and 88,642·74 s.

we may assume that the planet's rotation-period is fairly represented by the value

$22^{h}\ 37^{m}\ 22{\cdot}735^{s}$,

with a probable error of $\cdot005^{s}$ and a possible error of 015^{s}. More than this error, which gives a range of $\cdot03^{s}$ for each rotation, and therefore $36^{m}\ 6^{s}{\cdot}96$ for the total number of rotations since the date of Hooke's observations, it would be unreasonable to admit, since the planet's change of appearance, even in a quarter of an hour, is perfectly cognisable by a practised observer.

Kaiser's period ($22^{h}\ 37^{m}\ 22{\cdot}62^{s}$) which differs from mine by $0{\cdot}115^{s}$, would throw the planet out, at the period of Hooke's observations, by an amount corresponding to the amount of rotation during $2^{h}\ 20^{m}$. This is not to be thought of for a moment. Hooke depicts the Kaiser Sea at a distance of rather more than 18° from the centre, and the change resulting from Kaiser's period would throw the Kaiser Sea nearly 50° from the centre. At this distance it would have been lost in the haze near the planet's limb, even had Hooke's telescope been as powerful as the best modern instruments. On the other hand, the fact that Kaiser was led to the period $22^{h}\ 37^{m}\ 22{\cdot}62^{s}$ by the comparison of some of the best modern drawings, taken over a widely extended period, suffices to prove that Hooke's drawing depicts the Kaiser Sea, and not the Dawes' Strait, which sometimes looks not unlike the Kaiser Sea, especially at that season of the Martial year in which Hooke observed the planet. For Kaiser's period throws the Dawes' Strait almost exactly in the centre of that hemisphere of Mars which would have been turned away from Hooke at the time of his observation.

The period deduced by Messrs. Beer and Mädler, $22^h\ 37^m\ 23{\cdot}8^s$, is disproved by Sir W. Herschel's drawings; otherwise it would fairly account for Hooke's drawing on the omission of a complete rotation. Herschel's views coming midway between Hooke's and modern drawings satisfactorily remove any doubt of this sort.

Abridged from the *Monthly Notices of the Royal Astronomical Society* for Oct. 1867, Jan. 1868, and April 1869.

APPENDIX B.

NOTE ON THE SUN'S MOTION IN SPACE

AND ON THE RELATIVE DISTANCES OF THE FIXED STARS OF VARIOUS MAGNITUDES.

HAVING recently had occasion to examine Mr. Main's 'Table of the Proper Motions of 1,167 Stars,' and the conclusions with reference to the Sun's motion deduced from that table by a method devised by the Astronomer Royal, and carried out at his request by Mr. Dunkin, I have been led to notice certain facts which seem to me to be not without significance.

In the first place, I would call attention to the table drawn up by Mr. Dunkin, in which the sums of the squares of the stars' proper motions are compared with the corresponding sums when the proper motion of each star has been corrected for the Sun's calculated motion in space. Mr. Dunkin comments on the singular smallness of the correction thus introduced. And this view seems abundantly justified by the table, which runs as follows, the divisions one to seven corresponding to Struve's arrangement of the stars in order of magnitude :—

Sums of Squares of Motion in Parallel.

	Uncorrected.	Corrected.
Division 1	2·0637	1·2123
„ 2	1·8743	1·9292
„ 3	9·2894	9·5607
„ 4	6·4732	5·4608
„ 5	43·4126	42·4236
„ 6	14·8637	14·2750
„ 7	0·7814	0·7215
Sum	78·7583	75·5831

Sums of Squares of Motion in N.P.D.

	Uncorrected.	Corrected.
Division 1	6·7231	5·6883
„ 2	0·5351	0·7805
„ 3	4·9739	4·7569
„ 4	6·9390	6·5255
„ 5	39·4335	38·7292
„ 6	4·5671	4·3376
„ 7	0·0951	0·0904
Sum	63·2668	60·9084

Commenting on this result, Sir John Herschel remarks: 'No one need be surprised at this. If the Sun move in space, why not also the stars? And if so, it would be manifestly absurd to expect that any movement could be assigned to the Sun, by any system of calculation, which would account for more than a very small portion of the totality of the observed displacements.'

It had always seemed to me that this conclusion might require to be modified if the question were subjected to mathematical scrutiny; my reason for forming this view being this—that the largeness of the number of stars operates as much to increase the extent of the correction as to increase the amount of the uncorrected squares, since every star is affected by the Sun's motion in space.

It occurred to me recently that the following simple geometrical proof serves to show that the correction to be looked for is much larger than that which Mr. Dunkin's figures exhibit.

Suppose that in any small region of the sky a large number of stars (say n), all at the same distance from the Earth, are travelling in all directions with a velocity exactly equal to that with which the Sun is travelling. Let us suppose that the motion of any one of these stars which is travelling at right angles to the line of sight carries the star over an arc p in a year; and that the region of the sky is so situated that the effect of the Sun's motion on a star at rest (at the given distance) would be to produce an apparent annual proper motion q'.

$$\begin{aligned} &\text{Let A B (fig. 19)} = p \\ &\text{C D} \qquad\qquad = q' \end{aligned}$$

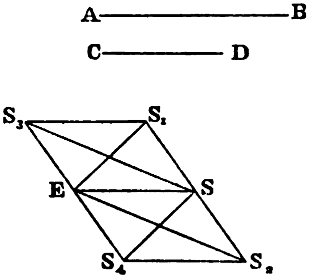
Suppose two of the stars to move in opposite directions, S S_1, S S_2, along the same straight line, both (we may suppose for convenience) starting from S, and the lines S S_1, S S_2, being in reality equal to A B, but foreshortened. Then the sum of squares for these stars, if unaffected by the Sun's motion, would be

$$= S S_1^2 + S S_2^2,$$

where $S S_1$, $S S_2$, represent the *apparent* lengths of these lines.

Suppose that S E is the apparent motion due to the Sun's real motion, so that $S E = C D$.

FIG. 19.



Complete the parallelograms $S S_1 S_3 E$ and $S E S_4 S_2$, and draw their diagonals. Then the sum of squares for the two stars as affected by the Sun's motion,

$$= S S_3^2 + S S_4^2$$
$$= S_2 E^2 + S_1 E^2$$
$$= S S_1^2 + S S_2^2 + 2 E S^2$$

Therefore, for every pair of stars moving in opposite directions, there is an increase of $2 q'^2$ in the sum of squares. Therefore there is an average increase of q'^2 for each star in the region.

Now a moment's consideration will show that, precisely as the lines $S S_1$ and $S S_2$ vary from the full length A B to zero, as we vary the supposed motion in all directions around S (for stars in a given small region of the heavens), so does C D vary from A B to zero as we shift the small region over the heavens. It is not merely that the limits of change are the same, but the proportion of lines of a given length $S S_1$ for the star's motion is exactly the same as the proportion of corrections equal to $S S_1$ for the Sun's motion. Since, therefore, for each region there is an increase per star equal to the square of the proper motion due to the Sun's motion (for that region), and the several stars of each region have proper motions varying according to exactly the same law as that according to which the effect of the Sun's motion varies over the celestial sphere, it is obvious that for the whole sphere the effect of the Sun's motion must be to increase the sum of squares by an amount exactly equal to the sum of squares due to the stars' own motions. In other words, the sum of squares is doubled through the effect of the Sun's motion; if only the Sun be assumed to travel at the same rate through space as the several stars at a given distance from him.

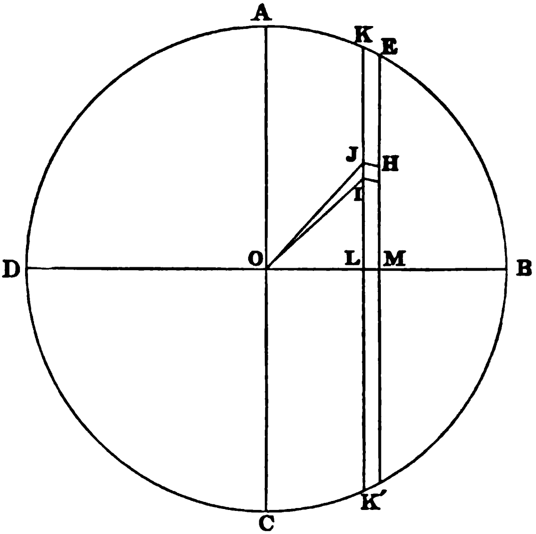
It is obvious also that the same is true if the stars at a given distance move with different velocities, but the Sun's velocity is the mean (its square equal to the mean of sum of squares) of the velocities of the stars at said distance.

Further, if the mean velocity of the stars at a given distance be p, and the Sun's velocity be q, then we have the ratio

$$\frac{\text{Corrected sum of squares}}{\text{Uncorrected sum}} = \frac{p^2}{p^2+q^2}.$$

Before proceeding to apply this law, I propose to deduce it by another process, which will serve to indicate what is the actual sum of squares, corrected or uncorrected, for a given large number of stars, all at the same distance from the Earth.

Fig. 20.



Let A B C D, Fig. 20, represent a small circular space on the celestial sphere; and suppose a large number of stars within this space, travelling in all directions with a mean velocity which would give to one moving at right angles to the line of sight an annual proper motion $= p$. Then, in order to determine the mean square of the apparent proper motion—the Earth being supposed to be at rest—we may suppose every star to start from O, O B $= p$, and that the points towards which the stars are severally moving are uniformly spread over the small sphere A B C D.

Take now a thin zone of the sphere's surface by parallel planes perpendicular to O B, through L K, M E, where

$$\text{O L} = x, \qquad \text{O M} = x + \delta x.$$

Then the number of stars whose directions lie towards some part of this zone, is (by a well-known property of the sphere),

$$= \frac{n \, . \, \delta x}{2 p},$$

n being the total number of stars.

Again, take planes B O I, B O J, inclined at angles θ and $\theta + \delta\theta$ to the plane through O B and the observer's eye.

Then the number of stars whose direction is towards some part of the small element I H of the sphere's surface

$$= \frac{n \, \delta x}{2 p} \cdot \frac{\delta \theta}{2 \pi}.$$

When the element I H is taken small enough, each of these stars has an apparent proper motion, whose square

$$= x^2 + (p^2 - x^2) \sin^2 \theta.$$

Therefore, it follows that the mean value required

$$= \frac{1}{p \pi} \int_0^{\frac{\pi}{2}} \int_{-p}^{+p} \left\{ x^2 + (p^2 - x^2) \sin^2 \theta \right\} d\theta \, dx$$

$$= \frac{2 p^2}{\pi} \int_0^{\frac{\pi}{2}} \left(\frac{\cos^2 \theta}{3} + \sin^2 \theta \right) d\theta$$

$$= \frac{2 p^2}{3 \pi} \int_0^{\frac{\pi}{2}} \left\{ \frac{1 + \cos 2\theta}{2} + \frac{3 (1 - \cos 2\theta)}{2} \right\} \delta\theta$$

$$= \frac{2 p^2}{3}; \tag{1}$$

and the sum of squares is therefore

$$= \frac{2 n p^2}{3}. \tag{2}$$

Next let us inquire what the sum would be if the solar system were moving in such a manner that each star within the circle A B C D was affected by a proper motion q (in any direction) due to the Sun's motion.

Let the motion be parallel to O B, and from right to left. Then it is clear that the only change in the expression to be integrated, is, that for x^2 we must write $(x-q)^2$. Therefore the mean square will be

$$\frac{2p^2}{3}+\frac{1}{p\pi}\int_0^{\frac{\pi}{2}}\int_{-p}^{+p}(q^2-2qx)\,d\theta\,dx$$

$$=\frac{2p^2}{3}+q^2. \qquad (3)$$

In this case, then, the sum of squares is

$$=\frac{2np^2}{3}+nq^2. \qquad (4)$$

Lastly, we have to inquire what is the sum of squares for N stars scattered over the whole of the celestial sphere at a given distance R, each affected by the proper motion *p in space*, the Sun being affected by a motion P (*p* and P representing the arc-motions due to these respective proper motions, when supposed to be taking place in a direction at right angles to the line of sight, and to be seen from a distance R).

In Fig. 20, let A B C D now represent the celestial sphere, and take

$$\text{O L}=y$$
$$\text{L M}=\delta y;$$

then the effect of the Sun's motion upon all stars in the zone K M K′ will be to affect them with a proper motion,

$$\text{P}\sin\cos^{-1}\left(\frac{y}{\text{R}}\right)$$
$$=\text{P}\sqrt{\left(1-\frac{y^2}{\text{R}^2}\right)}.$$

Hence, by (2) the average proper motion of stars in this band

$$=\frac{2p^2}{3}+\text{P}^2\left(1-\frac{y^2}{\text{R}^2}\right);$$

and, therefore, for the whole sphere, the average proper motion,

$$=\frac{1}{2\text{R}}\int_{-\text{R}}^{+\text{R}}\left\{\frac{2p^2}{3}+\text{P}^2\left(1-\frac{y^2}{\text{R}^2}\right)\right\}dy$$
$$=\frac{2p^2}{3}+\text{P}^2-\frac{\text{P}^2}{3}$$
$$=\frac{2}{3}(p^2+\text{P}^2). \qquad (5)$$

and the sum of squares of proper motions

$$=\frac{2\,N}{3}\,(p^2+P^2) \qquad (6)$$

If $P=p$ (that is, if the Sun's motion be assumed equal to the average motions of the stars) the sum of squares

$$=\frac{4\,N}{3}\,.\,p^2. \qquad (7)$$

The use of the integral calculus, as above, would only be justified where N is infinite; but where N is considerable, the result must be a close approximation to the truth, with the assumed conditions. And even where N is small, the above result is the most probable, in the case of a random distribution of the N stars, and of the directions of their motion.

Now, if we apply these results to the tables given above, we can determine how far the observed proper motions of the stars are consistent with the supposition that the Sun's proper motion is not very different from the mean proper motion of the stars of different magnitudes; and thence we can form an opinion as to the justice of those estimates of the stars' distances on which the values of the corrections tabulated above have been determined.

First, we require the sums of squares of proper motions, without reference to direction. Since the square of a star's proper motion is equal to the sum of the squares of the star's proper motion in parallel and in N.P.D., we have only to add the respective sums of squares in the two tables given above, to deduce the following table:—

Sums of Squares of full Proper Motion.

	Uncorrected.	Corrected.	N.
Division 1	8·7868	6·9006	9
,, 2	2·4094	2·7097	55
,, 3	14·2633	14·3178	146
,, 4	13·4122	11·9863	238
,, 5	82·8461	81·1528	330
,, 6	19·4308	18·6126	368
,, 7	·0·8765	0 8119	21
Sum	142·0251	136·4917	1167

Let us first apply formulæ (2) and (6) (remembering that formula (2) is true for the whole celestial sphere). We have from them,—

$$\text{Uncorrected sum of squares} = \frac{2\,N}{3}\,(p^2+P^2),$$

$$\text{Corrected sum of squares} = \frac{2\,N\,.\,p^2}{3}.$$

Hence

$$P^2 = p^2 \left(\frac{\text{correction}}{\text{corrected sum}} \right).$$

Applying this successively to the several divisions, we obtain

For	Division	1	$P = p\,(\cdot 52)$
,,	,,	2	P has an imaginary value
,,	,,	3	P has an imaginary value
,,	,,	4	$P = p\,(\cdot 34)$
,,	,,	5	$P = p\,(\cdot 18)$
,,	,,	6	$P = p\,(\cdot 21)$
,,	,,	7	$P = p\,(\cdot 28)$

The results for the second and third divisions suffice to show that the numbers of stars which fall under these heads are insufficient for a satisfactory determination; and therefore, *à fortiori*, the number of stars in division 1 is insufficient. Hence the result $P = p\,(\cdot 52)$ must be dismissed as valueless. But even at this stage of the inquiry we begin to recognise that there must be something wrong about our assumptions. For even if the numbers 55 and 146 were not in themselves large enough to lead us to expect a satisfactory evaluation for stars in the second and third divisions, yet the fact that the Sun's motion in space has been correctly deduced from a smaller number of stars would justify such an expectation.

I should be led then to suspect from this evidence, that if divisions 1, 2, and 3 had been taken together, a more satisfactory conclusion would have been arrived at, notwithstanding the apparent necessity of assigning different distances to stars in these divisions. However, it is not possible to determine how far this suspicion is justified without going over Mr. Dunkin's labours afresh, with changed assumptions; and I have no leisure for the long processes of calculation this would involve.

Next we may notice that the results for divisions 4, 5, 6, and 7 are very far from satisfactory. I cannot think it credible that the real solution of the difficulty involved in the smallness of the observed corrections is to be found in the assumption that the mean motion of stars of the fourth magnitude is three times as great as the Sun's, the mean motion of fifth-magnitude stars nearly six times as great as the Sun's, and so on.

If we try the effect of diminishing the assumed distances of stars of the 4th, 5th, and 6th divisions, so as to accord with the observed relations, on the assumption that in reality $P = p$, we have (the assumed distances being 3·76, 5·44, and 7·86,* respectively) the following results:—

For Division 4 Distance = 3·76 × ·34 = 1·28
" " 5 " = 5·44 × ·18 = 0·98
" " 6 " = 7·86 × ·21 = 1·65

As division 7 includes but 21 stars, we cannot expect any trustworthy results from treating it in the same way.

The evidence thus far seems strongly opposed to accepted views respecting the distances of stars of the smaller visible magnitudes. A further inquiry on this point seems, therefore, suggested. And it is obvious that in the above table of sums of squares we have the means of testing how far the assumed distances of the stars of various orders accord with the observed proper motions. I make the assumption that the Sun's motion is equal to the average proper motion of the fixed stars. This assumption affects the actual, but not the relative, values of the distances which result from the following processes.

We have then formula (7) to deal with. Applying it to the successive orders of stars, *i.e.* putting $\frac{4\text{N}}{3}p^2$ successively equal to the values tabulated in the foregoing columns of uncorrected sums of squares, we obtain

	Apparent Proper Motion.	Resulting Distance.
	"	
Division 1	0·857	1
" 2	0·182	4·7
" 3	0·268	3·2
" 4	0.208	4·1
" 5	0·433	2·0
" 6	0·191	4·5
" 7	0·173	5·0

This result is unsatisfactory in the extreme. We find stars of the second magnitude placed further from us (according to this mode of estimating their distances) than stars of the 3rd, 4th, 5th, and 6th magnitudes.

Remembering the evidence we have already had, that (1) there is something erroneous in our assumptions respecting star-distances; and secondly, that small numbers of stars are insufficient for our guidance, let us apply a test which there ought to be no mistaking. Let us divide the stars into two sets, the first including divisions 1,

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* Struve's values.

2, 3; the second including the remaining divisions, and let us apply formula (7) to these sets.

We obtain,

For Set 1, Apparent Proper Motion,

$$=\sqrt{\frac{3}{4}\left(\frac{25\cdot4595}{210}\right)}=0''\cdot3015,$$

For Set 2, Apparent Proper Motion

$$=\sqrt{\frac{3}{4}\left(\frac{116\cdot5656}{957}\right)}=0''\cdot3022.$$

This result would make the mean distance of stars of the first three magnitudes equal to (or *very slightly less* than) the mean distance of stars of the next three magnitudes!

I am very far from supposing that this result accurately represents the relations subsisting among the stars; but I do think that it suffices to render the usually accepted views respecting stellar distribution wholly untenable. Remembering that whatever theory we form regarding the relation between the apparent brilliancy and the real distance of the stars, we must yet recognise the fact that the stars are at very various distances from us, I think it must be admitted that the apparent brightness of a star is, to a certain extent, an argument of relative proximity. A large proper motion is also an argument of relative proximity. If the two indications agreed either for separate stars, or for sets of stars arranged according to apparent brilliancy, there would be no difficulty. As we find, however, that there is no such agreement, we are forced to consider whether brightness or large apparent motion is the stronger evidence of proximity. Judging from the analogy of the solar system, in which the range in the variations of magnitude is enormously greater than the range in the variations of velocity, we seem strongly led to look on the proper motions of stars as in reality the best evidence we have respecting their distances. But this conclusion is very much strengthened when we remember that the dynamical conditions in the sidereal system must be much more unfavourable to the occurrence of wide variations of velocity, than the conditions which prevail in a system of bodies circling around a central body enormously large compared with any of its dependent orbs.

I think, then, that I may fairly look upon the above inquiry as affording very striking evidence in favour of the view I had formed from other considerations, that the assumed estimate of the distances of the smaller stars has been greatly overrated. And as this conclusion may obviously be extended to yet smaller stars, I think that I have not been deceived in looking upon the relations which subsist between the Milky Way and the lucid stars in its neighbourhood as very much more intimate than has been commonly supposed. I believe that future researches will prove, not only that the Milky Way as a whole is much nearer than we have been imagining, but that portions of it are absolutely nearer to us than the brightest of the single stars. That parts of the Milky Way, for instance, in the neighbourhood of α Centauri (the nearest of the fixed stars, so far as is yet known), are nearer to us than that star, I think the whole aspect of the galaxy in that neighbourhood suffices to suggest, if not to demonstrate.

Monthly Notices of the Royal Astronomical Society for November 1870.

APPENDIX C.

NOTE ON THE TRANSIT OF VENUS IN 1874.

AND AN EXACT DETERMINATION OF THOSE POINTS ON THE EARTH'S SURFACE AT WHICH INTERNAL CONTACTS ARE MOST ACCELERATED AND RETARDED BY PARALLAX. WITH AN ADDENDUM REFERRING TO THE POSSIBILITY OF DETERMINING THE SOLAR PARALLAX BY THE SAME SORT OF OBSERVATIONS IN 1874 AS WERE MADE IN 1769.

Having applied geometrical tests to the elements discussed in the paper and maps by the Astronomer Royal ('Monthly Notices,' December 11, 1868), I was led to believe that some of the corrections which would result from an exact calculation of the circumstances of the two Transits of Venus, would be larger than a first view of the subject might suggest. I now present some of the results to which my examination of the Transit of 1874 has led me, reserving for a future occasion the examination of the Transit of 1882.

I premise that internal contacts are the phenomena to be specially considered; so that, although I have calculated the position angles for external contacts and passages of Venus's centre, I have not thought it necessary to determine the exact points on the Earth's surface at which these phenomena are most affected by parallax. I must also add that I have taken as the basis of my calculation the following elements calculated by Mr. Hind, and obligingly supplied me by the Astronomer Royal.

Transit of Venus, December 8, 1874.

For the centre of the Earth.

Ingress.

	h m s	
External contact	13 46 56	Greenwich Mean Solar Time.
Internal contact	14 15 57	" "

Egress.

	h	m	s	
Internal contact	17	57	5	Greenwich Mean Solar Time.
External contact	18	26	5	„ „

The elements sent me also contained the position-angles for external contacts, but as these were only given to tenths of degrees, I preferred to recalculate them.

From an examination of these elements I learn that the estimated diameter of Venus is about 7,364 miles, corresponding to an apparent semidiameter of 8″·305 at the Earth's mean distance, and an assumed equatorial horizontal solar parallax of 8″·94.

The Sun's longitude on December 8, 1874, at 16h, I estimate at 256° 57′ (with sufficient approximation); the apparent inclination of the Sun's ecliptical diameter to a declination-parallel 5° 36′ (the western end raised towards the north); the Sun's declination 22° 49′ S.

The formulæ I have used are founded on the following considerations. Conceive the Sun and Earth enclosed in a cone whose vertex is beyond the Earth, and also in a double cone whose vertex lies between the Earth and the Sun. These cones will have a common axis—namely, the Earth's radius vector. Now, it is obvious that when Venus has reached the surface of the outer cone, a transit commences at a certain point on the Earth's surface. As soon as Venus has reached the surface of the inner cone, transit has begun for all places on the Earth's illuminated hemisphere. The former contact corresponds to the case of ingress accelerated by parallax, the latter to the case of ingress retarded by parallax. Similar considerations apply to egress as affected by parallax.

It is clear that if we suppose the Earth's radius-vector reduced to rest and the motion of Venus correspondingly retarded, the remaining motion of Venus is that with which she may be assumed to traverse the section of the pair of cones above described. Thus it is easy to calculate the duration of a central passage for external and internal contacts, or for the centre of Venus, and either for the outer or inner cone, or for a cone between the two having the Earth's centre for vertex. The case of external contacts for the last-mentioned cone gives the duration of a central transit as seen from the Earth's centre; and by comparing this duration with that given in Mr. Hind's elements, we can tell how far the transit of 1874 differs from a central one.

On such considerations the following formulæ and calculations are founded:—

Let D = Sun's diameter.
d = Earth's mean distance.
d' = Venus's mean distance.
r = Earth's actual distance.
r' = Venus's actual distance.
v = Earth's mean velocity.
v' = Venus's mean velocity.
Δ = Earth's mean diameter.
Δ' = Venus's mean diameter.
i = inclination of Venus's orbit.

Then $v' = v\sqrt{\frac{d}{d'}}$; the actual velocity of the Earth is $v\frac{d}{r}$ (very nearly); that of Venus is $v\sqrt{\frac{d}{d'}} \cdot \frac{d'}{r'} = v\,\frac{\sqrt{d\,d'}}{r'}$; velocity of the Earth's radius vector at Venus's distance is $v\frac{d\,r'}{r^2}$. Whence it readily follows that the actual velocity with which Venus crosses the section of the cones described above is

$$= \sqrt{\left\{\frac{\sqrt{d\,d'}}{r'} - \frac{d\,r'}{r^2}\cos i\right\}^2 + \left\{\frac{d\,r'}{r^2}\sin i\right\}^2}$$

$$= \sqrt{\left(d^2\,r'^4 + d\,d'\,r^4 - 2\,d^{\frac{3}{2}}\,d'^{\frac{1}{2}}\,r^2\,r'^2\cos i\right)} \div r^2\,r'$$

$$= V, \text{ suppose.}$$

Also the cross-section of the cone whose vertex is at the Earth's centre is $D\left(\frac{r-r'}{r}\right)$. Therefore, the duration of a central transit of Venus's centre

$$= \frac{1}{V}\left\{\frac{D\,(r-r')}{r}\right\};$$

the duration of a central transit for external contacts

$$= \frac{1}{V}\left\{\frac{D\,(r-r')}{r} + \Delta'\right\};$$

the duration of a central transit for internal contacts

$$= \frac{1}{V}\left\{\frac{D\,(r-r')}{r} - \Delta'\right\};$$

all these phenomena being supposed to be seen from the Earth's centre. To determine the interval between phenomena affected by parallax, we have, the section of the outer cone mentioned above

$$= \frac{D(r-r')}{r} + \frac{\Delta r'}{r};$$

the section of the inner cone mentioned above

$$= \frac{D(r-r')}{r} - \frac{\Delta r'}{r};$$

and it is readily seen that, for a central transit, the interval between first external contact most accelerated by parallax and last external contact most retarded, is

$$= \frac{1}{V}\left\{\frac{D(r-r')}{r} + \frac{\Delta r'}{r} + \Delta'\right\};$$

the interval between first external contact most retarded by parallax and last external contact most accelerated, is

$$= \frac{1}{V}\left\{\frac{D(r-r')}{r} - \frac{\Delta r'}{r} + \Delta'\right\};$$

the interval between first internal contact most accelerated by parallax and last internal contact most retarded, is

$$= \frac{1}{V}\left\{\frac{D(r-r')}{r} + \frac{\Delta' r'}{r} - \Delta'\right\};$$

and, lastly, the interval between first internal contact most retarded by parallax and last internal contact most accelerated, is

$$= \frac{1}{V}\left\{\frac{D(r-r')}{r} - \frac{\Delta r'}{r} - \Delta'\right\};$$

By taking the recognised values of D, Δ, Δ', v, d, d', and i; $r = \cdot 98480$ (the Earth's mean distance being unity), and $r' = \cdot 72040$, all the above intervals are readily calculated. The duration of a central transit of Venus's centre, seen from the Earth's centre, is found to be $7^{\text{h}}\ 55^{\text{m}}\ 8 \cdot 256^{\text{s}}$. A similar transit calculated for external contacts has a duration of $8^{\text{h}}\ 10^{\text{m}}\ 25^{\text{s}} \cdot 140$, or 8·17365 hours. Since the duration of the transit of 1874, estimated for external contacts, is 4·6525 hours, we have for determining the arc 2θ of which the actual path of Venus is the chord,* the equation

$$\sin\theta = \frac{4 \cdot 6525}{8 \cdot 1736} = \sin 34^\circ\ 41'\ 40'' \text{ approximately};$$

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* The arc and chord here referred to belong to a circle having a diameter of $\left\{D\frac{(r-r')}{r} + \Delta'\right\}$; but the position-angles eventually deduced may, of course, be referred to the Sun's disc.

and it is clear that the distance separating the chord of transit from the centre of the section is

$$= \frac{1}{2}\left\{\frac{D(r-r')}{r} + \Delta'\right\}\cos\theta.$$

This distance is a constant in the calculation, and it will be convenient to call it k.

Now, if we want to determine the duration (t) of the transit of Venus across a section of any other radius, as s, and the corresponding arc of transit ($2\,\phi$) we have the formulæ

$$\cos\phi = \frac{k}{s}\,;\ \text{and}\ t = \frac{2\,s\sin\phi}{V}$$

We must give to $2\,s$, successively, the four values included in the expression

$$\left\{\frac{D(r-r')}{r} \pm \Delta\frac{r'}{r} \pm \Delta'\right\}.$$

In this way the durations were calculated which correspond to the epochs given in the accompanying table. For deducing position-angles from the arcs of transit, we have—

The apparent angle at which Venus is ascending from the ecliptic towards the west

$$= i + \tan^{-1}\left\{\frac{d\,r'^2\sin i}{\sqrt{d\,d'}\,r^2 - d\,r'^2\cos i}\right\} = 9^\circ\,9'$$

The angle at which the ecliptical diameter of the Sun is inclined to a declination-parallel (west end raised towards the north)

$$= 5^\circ\,36'.$$

Therefore the apparent angle at which Venus is crossing a declination-parallel, rising towards the west, $= 14^\circ\,45'$.

Thence it readily follows that the arc of transit $2\,(34^\circ\,41'\,40'')$ corresponds to the following position-angles,—

External contact at ingress, 130° 33′ from S. through E. towards N.
" " egress, 160° 4′ " " W. " "

These values correspond satisfactorily with those given by Mr. Hind, viz. 130°·6 and 160°·0. The same angle 14° 45′ applied to the transit-arcs obtained in the other cases examined, gives the position-angles in the second column of the following table:—

Elements of the Transit of Venus, December 8, 1874.

	G. M. T.			Position angle.		
	h	m	s	°	′	
First external contact, most accel. by par.	13	37	35	128	48	from S to E.
„ „ from Earth's centre	13	46	56	130	33	„
„ „ most retarded by par.	13	57	47	132	41	„
First internal contact, most accel. by par.*	14	3	59	133	56	„
„ „ from Earth's centre	14	15	57	136	28	„
„ „ most retarded by par.*	14	29	5	139	26	„
Last internal contact, most accel. by par.*	17	43	58	168	57	from S. to W.
„ „ from Earth's centre	17	57	5	165	59	„
„ „ most retarded by par.*	18	9	4	163	27	„
Last external contact, most accel. by par.	18	15	15	162	12	„
„ „ from Earth's centre	18	26	5	160	4	„
„ „ most retarded by par.	18	35	26	158	19	„

To which may be added,

First passage of ♀'s centre across Sun's limb	14	0	51	133	17	from S. to E.
Last „ „ „	18	12	11	162	48	from S. to W.

(both as seen from the Earth's centre).

The phenomena marked * are those which now concern us. We notice, first, that the maximum interval of time to be determined by the observations applied to the ingress and egress (internal contacts) is $25^m\ 6^s$. This is the interval which would result if each observation were made when the Sun is only raised above the horizon through the effects of refraction. A somewhat longer interval might be dealt with if the effects of refraction were considered, and if the observation of contact were possible when the Sun is close to the horizon. But we need not inquire into the possible extent by which the interval might thus be increased, since observations when the Sun is on the horizon are not available in the present instance. Practically we may consider that an interval of about 20^m has to be dealt with, or at the outside 22^m. As this interval is directly proportional to the solar parallax we can estimate the effect of errors of observation, or of error in the determination of the longitude of a station. An error of one second in the determination of an interval of 21^m would correspond to an error of about $0''\cdot007$ in the determination of the solar parallax.

(In an addendum, I propose to deal with the method of determining the solar parallax by difference of durations, so far as this method is applicable to the transit of 1874. Unless I mistake, I shall be able to show that more is to be hoped from the latter method, than from the one which is the object of the present investigation.)

To return to the consideration of the actual circumstances under which ingress and egress are most affected by parallax. To determine the longitude and latitude of the four points on the Earth's surface at which the acceleration and retardation have their maximum values, we have the following formulæ—

Let P = the position-angle, measured from the south point.
δ = Sun's declination.
e = equation of time (additive to mean time).
h = hour at which a phenomenon (ingress or egress) occurs.
l = latitude of point required.
λ' = longitude of point required (λ' being estimated from the meridian along which it is apparent noon at the epoch of the phenomenon).

Then $\sin l = \cos P \cos \delta$; and $\cot \lambda' = \cot P \sin \delta$.

I give the formulæ in this form as being that I have made use of. Strictly speaking, a reference should be made to the signs of l, λ'; but the circumstances of each case are quite sufficient to clear up any doubts which might arise. It is obvious that for the first and fourth phenomena marked * in the above table, the latitude is north, and for the others south; because, referring to the preliminary considerations it is seen that the tangent-lines corresponding to the former pair of phenomena belong to a cone whose vertex is outside the Earth, whereas the tangent-lines corresponding to the other phenomena belong a cone whose vertex lies between the Earth and the Sun. From similar considerations it follows that the longitudes in the first and third cases are eastward of the meridian along which it is noon at the respective epochs, whereas, in the second and fourth cases, the longitudes are westward of the corresponding meridian. With this relation in view, the actual longitude (λ) east of Greenwich is given by the expression

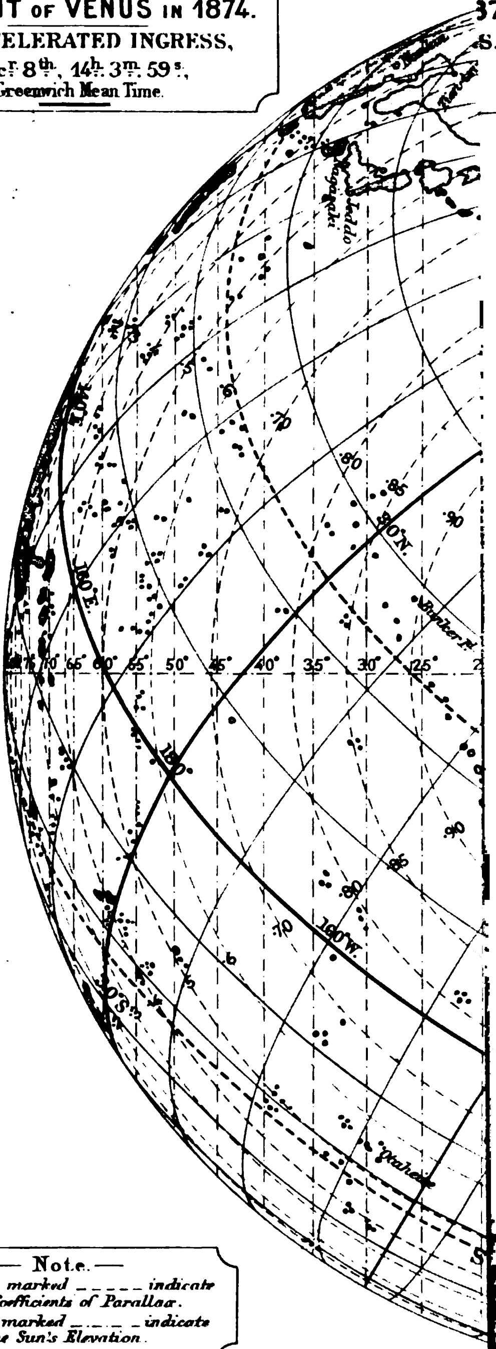
$$\lambda = 24^{h} - (h + e) \pm \lambda'$$

(*Note.* e changes by a few seconds during the progress of the transit.)

TRANSIT OF VENUS IN 1874.

1. ACCELERATED INGRESS,

Decr. 8th., 14h. 3m. 59s.,
Greenwich Mean Time.



— Note. —

The Circles marked _ _ _ _ _ indicate the Coefficients of Parallax.

The Lines marked _ . _ . _ _ indicate the Sun's Elevation.

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Thus we obtain the following results:—

	Lat.	Long.
	° ′	° ′
(i) The place at which first internal contact is most accelerated lies in . . .	39 45 N.	143 23 W.
(ii) The place at which first internal contact is most retarded lies in	44 27 S.	26 27 E.
(iii) The place at which last internal contact is most accelerated lies in . . .	64 47 S.	114 37 W.
(iv) The place at which last internal contact is most retarded lies in	62 5 N.	48 22 E.

On a comparison of these positions with those marked in the Astronomer Royal's maps, it will be noticed that the agreement is much closer for the places which fall in north latitude (that is, cases I. and IV.) than for the other two. Indeed, as respects the two former positions there is little to affect the general conclusions which the Astronomer Royal has deduced from geometrical considerations. In case I. (first map of Plate VI.) the point of maximum acceleration has been removed some 280 miles towards the north-west, rather westerly; and in case IV. (second map of Plate VII.) the point of maximum retardation has been removed some 220 miles towards the north-east.

It is in cases II. and III. (see the other maps of Plates VI. and VII.) that the most important changes have been made. In the first the point of maximum retardation has been removed to a distance of upwards of 800 miles towards the south-west. This change is unfavourable so far as the choice of stations for observing the retarded egress is concerned. Fort Dauphin in the Island of Madagascar (where, however, the Sun would be low at the time of internal contact at ingress) and Prince Edward and Marion Islands, are the places which, with Crozet's Island and Kerguelen's Land, are best placed for this observation.

In case III. the change which has been made is not quite so large. The point of maximum acceleration has been shifted nearly 700 miles towards the south-east, rather easterly. This change is also unfavourable, as it removes the point further away from all the places mentioned by the Astronomer Royal. It follows that Auckland Island, Emerald Island, and Victoria Land, are the places best situated for observing the egress accelerated by parallax.*

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* The results of calculations similar to the above, applied to the transit of 1882, are as follows: —

[The elements marked * being deduced from the formulæ discussed in my

A portion of the corrections which have here been mentioned are due to the choice of internal contacts as the phenomena to be chiefly attended to. I believe no doubt can exist that this choice is a just and proper one. Indeed, so far as I am aware, no calculations for the determination of solar parallax have ever been founded on attempts to determine the epoch of external contacts.

Since the above was written I have re-examined the whole subject by a new method, with results strictly accordant with those above obtained. But additional inquiry into the circumstances of the transit of 1874, with special reference to the question of dura-

paper in the March number of the 'Notices,' and the others being Mr. Hind's fundamental elements of the transit] —

		G.M.T.			Position-angle. From S. to E.
		h	m	s	°
First External Contact	(from Earth's centre)	1	55	38	34·7
*First Internal Contact	(most accelerated by parallax)	2	7	51	32·6
„ „	(from Earth's centre)	2	15	56	
* „ „	(most retarded by parallax)	2	24	18	29·8
					From S. to W.
*Last Internal Contact	(most accelerated by parallax)	7	44	5	61·3
„ „	(from Earth's centre)	7	52	27	
* „ „	(most retarded by parallax)	8	0	32	64·1
Last External Contact	(from Earth's centre)	8	12	47	66·1

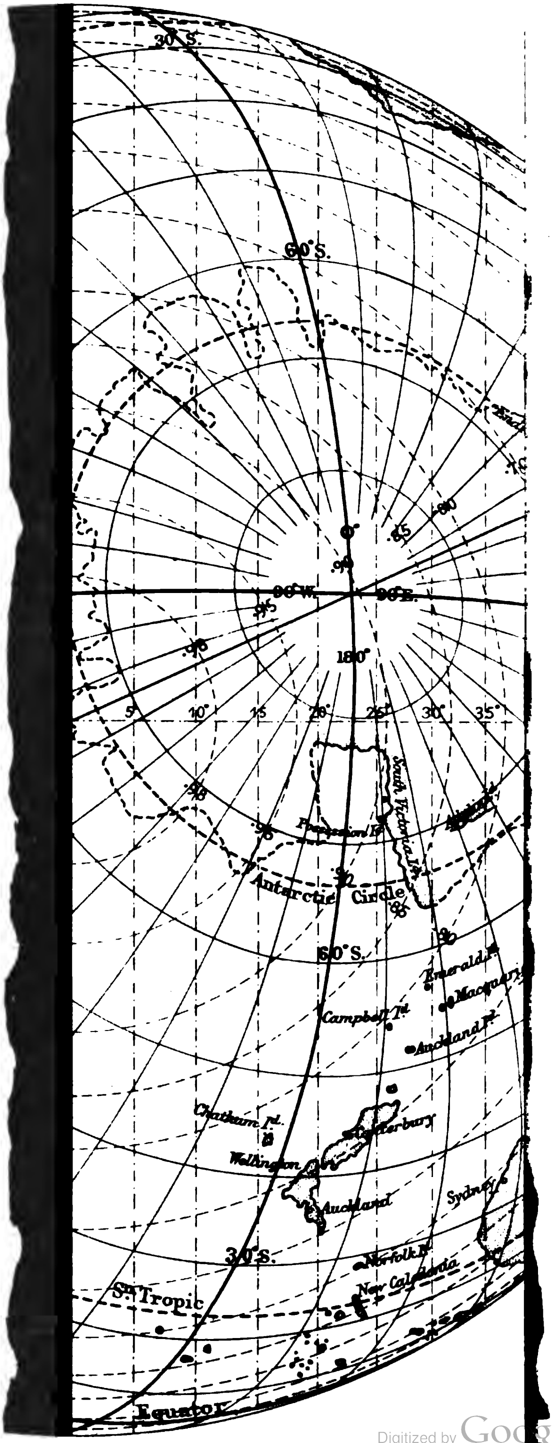
The calculated positions of the places where ingress and egress are most affected by parallax, are as follows: —

	Lat.			Long.		
	°	′		°	′	
(i) The place where first internal contact is most accelerated, lies in	51	6	S.	86	48	E.
(ii) The place where first internal contact is most retarded, lies in	53	16	N.	94	28	W.
(iii) The place where last internal contact is most accelerated, lies in	26	18	N.	40	1	W.
(iv) The place where last internal contact is most retarded, lies in	23	46	S.	137	11	E.

Not one of these places differs by much more than 300 miles from the corresponding place obtained by considering the passage of Venus's centre, with the position-angle for external contact,—the phase with reference to which these places had hitherto been calculated.

The transit of 1882 has an interest which that of 1874 wants, in being partially visible in England.

Plate VII.



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tion, and for the case of internal contacts, has led me to still clearer evidence of the value of this transit. I have constructed the orthographic projections of the Earth for the epochs of first and last internal contacts as seen from the Earth's centre. Across these projections I have laid down the lines which mark the order of the occurrence of internal contacts as seen from different parts of the Earth's surface, from minute to minute. Thus, in the first projection I have a series of twenty-six lines commencing with a tangent to the Earth's north-easterly quadrant, 133° 56′ from the north, and ending with a tangent to the Earth's south-westerly quadrant, 139° 26′ from the south: in the second I have a similar number of lines commencing with a tangent to the Earth's south-easterly quadrant 165° 59′ from the north, and ending with a tangent to the Earth's north-westerly quadrant 160° 4′ from the south. These lines enable one at once to recognise the best stations for observing the transit as most lengthened and as most shortened by parallax.

I find that any part of a nearly circular region extending from Lake Baikal to the southern part of Saghalien Island, and from north latitude 40° to north latitude 60° gives a well-lengthened transit, while the northernmost part of this region gives a lengthening of about $16\frac{1}{4}^{m}$. For southern stations there is a wide range of choice, though only three or four stations give nearly the full available shortening. I find that Petra Island (assuming it rightly placed in my atlas, viz. in west longitude 88°, and south latitude 71°), gives a transit shortened by 20^{m}. The place near Repulse Bay mentioned by the Astronomer Royal as suitable for the observation of the transit of 1882, gives in 1874 a shortening of $18\frac{1}{2}^{m}$. The place near Victoria Land similarly referred to by Mr. Airy, gives a shortening of $18\frac{3}{4}^{m}$ in 1874. A place on Enderby Land will give a shortening of no less than $20\frac{1}{4}^{m}$. Crozet's Island and Kerguelen's Land are also suitable, giving transits shortened by 17^{m} and 16^{m} respectively. Macquarie Island, Royal Company Island, Hobart Town, and even parts of New Zealand, would serve as useful subsidiary stations.

The best southern station taken in conjunction with the best northern station gives a difference of duration of no less than $36\frac{1}{2}^{m}$. The maximum of difference in 1882 will be about 28^{m}. This is founded on the Astronomer Royal's statement that Sabrina Land and Bermuda give a difference which is to the absolute maximum of difference ($32\frac{3}{4}^{m}$ for internal contacts) as 341 to 400. Thus if we assume the value of a transit to be directly proportional to the observable difference of duration, the transit of 1874 is more valuable than that of 1882 in the proportion of 36½ to 28, or more than 9 to 7. If, however, we hold that the slowness of ingress diminishes the value of a transit in precisely the same proportion that it increases the maximum difference of duration, the transit of 1874 is inferior to that of 1882 in the proportion of

$$\frac{36\frac{1}{2}}{50\frac{1}{5}} \text{ to } \frac{28}{32\frac{3}{4}} \text{ or almost exactly 6 to 7.}$$

Probably the truth lies between these results, so that we may assign to the transit of 1874 a value which bears to that of 1882 the ratio of

$$\tfrac{1}{2}\,(9+6) \text{ to } 7\text{, that is, } 15 \text{ to } 14.$$

Judged in either way the transit of 1874 is superior in value to that of 1769, *as actually utilised.*

In any case, it seems clear that the method founded on observed differences of duration is not inapplicable to the transit of 1874. On the contrary, when we remember all the difficulties in the way of the second method, the former seems to be the one from which the most satisfactory results are to be expected.

Monthly Notices of the Royal Astronomical Society for March 1869.

In the 'Monthly Notices' for March 1869, I indicated my intention of applying more exact modes of examination to the circumstances of the approaching transits of Venus, than had hitherto been considered necessary. In resuming the examination of the transit of 1874, I feel it necessary to call attention to that statement of my object, lest it should be thought that the corrections which I have been led to make on former results, point to errors in the processes by which those results were obtained, whereas, in reality, they are only due to the intentional neglect in prior work of considerations which even now many may be disposed to look upon as unnecessarily exact for the purposes of a preliminary inquiry. To still further obviate this objection (which, however, is not likely, I think, to occur to any one who has read my former papers), I shall for the most part refer to former results as those which have been obtained on such and such suppositions. This is the more advisable, because I find that astronomers are willing to accord the same degree of accuracy to my results in relation to the modes by which they have been obtained, as I am ready to assign to other results with a corresponding *proviso.*

It may be necessary, however, before proceeding to a detailed examination of the circumstances of the coming transit, to inquire (1) how far it is necessary or advisable to aim at exactness in the preliminary investigation of the subject; and (2) whether the phase which I have selected for examination is well chosen.

As a reason for applying a comparatively rough mode of examination to the circumstances of the transit, it has been urged that there is some uncertainty as to the exact moment of conjunction of Venus and the Sun, and as to the exact distance which will separate their centres. What is the range of possible error due to this cause I will leave to others to determine. My own impression is that it is very small, whether considered absolutely or with reference to other sources of error which it is in our power to remove. But whether this be so or not, it seems clear to me that the possible errors, as to time and position-angle, arising from this cause, must be looked upon as affording additional reason for exactness in other matters. For, should any error resulting from disregard of exact considerations happen to be *additive* to errors resulting from inaccuracy in the Planetary Tables, it might happen that the conclusions to which we should be drawn would affect appreciably the success of observations to be made upon the transit.

The possible inaccuracy of the Planetary Tables seems to be the only source of error which it is out of our power to get rid of altogether. Therefore, as the choice of stations depends in certain cases on considerations of some nicety, it seems to me that the only basis for a proper selection is the determination, with as much accuracy as is readily attainable, of the circumstances under which the phase selected for observation will be seen at different stations. It must be remarked, however, that the formulæ and calculations by which I have determined the circumstances are not by any means necessary. A process which I will presently indicate suffices to give, with scarcely any labour, all the necessary information respecting a large number of stations, and that with a degree of accuracy amply sufficient for all the purposes of the inquiry. The only way, however, in which I could conveniently present my results for the study of those who take interest in the subject of this transit was that which I have selected.

As to the selection of internal contacts as the phase to be specially dealt with, there may be, of course, a difference of opinion. I am told, for instance, that in France there is some question of observing external contacts also. In this case it would be advisable to make external contacts the determining phase (if a choice had to be made), or to determine for several stations the circumstances of both phases. For as at ingress external contact takes place about half an hour before internal contact (for every station), it is clear that the stations for observing retarded ingress (always so selected that the Sun is lately risen) must be chosen with reference to external contacts, if it is a *sine quâ non* that these are to be observed. If such a station were selected with reference to internal contact, with a moderately good solar elevation (necessarily *soon after* Sunrise), it is obvious that at the epoch of external contact half an hour earlier, the Sun would be too low. Considerations of the same sort apply to the observations of accelerated egress.

It appears to me for these reasons that the consideration of both phases is inadmissible, and that though external contacts may be observed with advantage when they happen to be visible *as well as* internal contacts, yet the stations must be chosen with strict reference to the latter phase, which is admittedly far the better of the two.

As M. Puiseux has given reasons for selecting the passage of Venus's *centre* over the Sun's limb as the phase to be considered in the preliminary inquiry, it may be well to examine how far those reasons are valid. He remarks that what is actually observed is the exterior or interior contact, but that, as the epochs of these phases are separated by nearly constant intervals for all stations, we may neglect the consideration of those intervals when all that we require is to determine the relative values of different stations. I venture to submit the following reasons for looking upon this line of argument as inadmissible :—

It is to be noted, in the first place, that the interval between external and internal contact is not constant, and although its range (from $26^m\ 24^s$ to $31^m\ 18^s$) may seem small, yet, where the values of stations run close, the differences thus resulting are quite sufficient to turn the scale in several instances. But we may also look at the matter in this light: the change of phase results in a change in the position of the stations of maximum acceleration, or retardation, and this change amounts in certain cases to hundreds of miles. Now the circles which give a definite coefficient of parallax in one of these cases,* have equal radii, but their poles (the stations just

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* The coefficient of parallax is the proportion which the total acceleration or retardation of the contacts of Venus, as seen at any given station, bears to the maximum acceleration or retardation respectively.

TRANSIT OF VENUS IN 1874.

5. MEAN INGRESS,

Decr. 8th, 14h. 15m. 57s.
Greenwich Mean Time.

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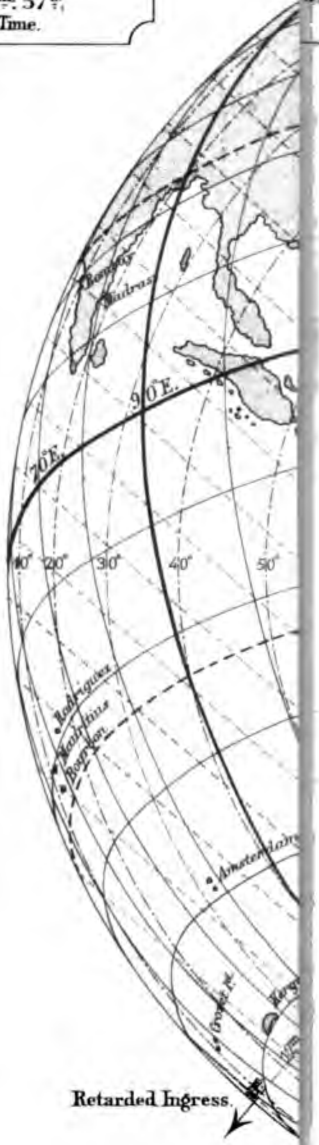

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mentioned) are separated by a considerable distance. Therefore the circles must intersect. At a point of intersection the coefficients are equal for either phase: on one side of that point, the nominal value of the coefficient is improved by the change of phase; on the other, it is deteriorated. This proves that the relative values of stations cannot remain unaffected by a change of phase, and suffices to account for the corrections introduced by my consideration of internal contacts in place of the passages of Venus's centre.

It would be incorrect, however, not to mention that the larger part of the corrections I now propose to consider, results from another cause than the change of phase—namely, from the effects of parallax—which it has not hitherto been thought necessary to take into account. The application of the correction due to the equation of time has also to be mentioned as appreciably affecting the extent of the changes which are here dealt with.

I may summarise these changes under three heads:—

1. *The application of Delisle's method of absolute time differences.* The relative as well as the absolute values of many stations are affected by the change of phase. Some which had hitherto appeared unsuitable are found to be unobjectionable. Others which seemed good appear unfit. In other cases the relative values of two stations are so affected that the results of a comparison between them are directly reversed. Lastly, many stations not hitherto thought of in connexion with the transit are found to be well suited for the application of Delisle's method.

2. *The comparison between Delisle's and Halley's methods.* Halley's method is found, not merely to be applicable with advantage, which is all that can be said of it when central passages are considered, but to be superior to Delisle's,—slightly, when reference is made only to such stations as had been hitherto dealt with, noticeably when antarctic stations are made use of.

3. *The comparison between the Transits of* 1874 *and* 1882 *with reference to Halley's method.* This comparison, conducted according to the principles laid down by Mr. Stone (than whom no one is better entitled to pronounce authoritatively on such points) shows that Halley's mode may be applied much more advantageously to the transit of 1874 than to that of 1882.

I premise that I have carefully gone over all the calculations on which the results of my former paper were founded, and have applied so many independent modes of calculation that I can no longer entertain any doubt as to the accuracy of those results.*

The results to be now brought into comparison for the sake of forming an estimate of the effect of phase, parallax, and the equation of time, upon the values of various stations, may be thus classed :—

A, those derived from the consideration of central passages, as supposed to be seen from the Earth's centre, with the position-angles corresponding to external contact.

B, those derived from the same phase, similarly seen, with the position-angles corresponding to central passage.

C, those derived from the consideration of internal contacts, as seen from the stations themselves, and with the position-angles corresponding to the phase so seen.

Note.—The results under head C have alone been corrected for equation of time.

The following table exhibits the position-angles and epochs (for ingress) corresponding to A, B, and C:—

	Position-angles.					Epochs.					
	A	B		C		A		B		C	
	°	°	′	°	′	h	m	h	m	h	m
Accelerated Ingress	131	133	29	133	56	14	0	13	55	14	4
Mean Ingress	131	133	29	136	28	14	0	13	55	14	16
Retarded Ingress	131	133	29	139	29	14	0	13	55	14	29

In preparing the columns under head C in the accompanying tables, I have made use of the six orthographic maps shewn in Plates VI., VII., VIII., and IX. These were constructed with every precaution to ensure accuracy. The intersection of longitude-lines and latitude-parallels (to every 10°) were separately constructed for, by a double process, and in all critical cases further tests were applied. In all, the construction of the maps involved upwards of 3,000 measurements, and from the scale on which the maps were constructed, the practice I have had in orthographic mapping, and the care used in the present instance to ensure accuracy, I feel that I may safely claim for the numbers under the columns headed C in the following tables, a satisfactory degree of trustworthiness.

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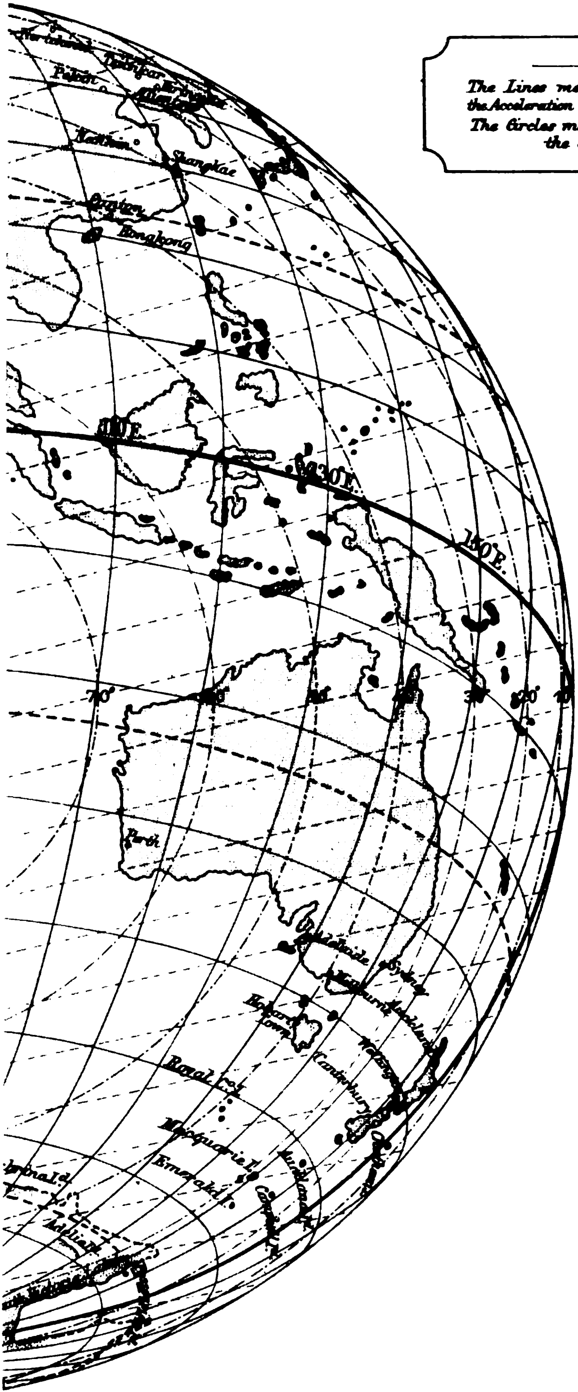
* This paper had originally exhibited an independent mode of establishing my conclusions, founded on a simple construction applied to the elements given in Mr. De la Rue's paper on the transit (*Monthly Notices* for December 1868). For the sake of brevity, I have thought it best to omit this part of the paper, the accuracy of my conclusions not being, I find, called in question. The mode of construction is, however, briefly referred to further on.

Plate IX.



Note.

The Lines marked - - - - - - indicate the Acceleration and Retardation in Minutes
The Circles marked — . — . — indicate the Sun's Elevation.

Accelerated Egress.

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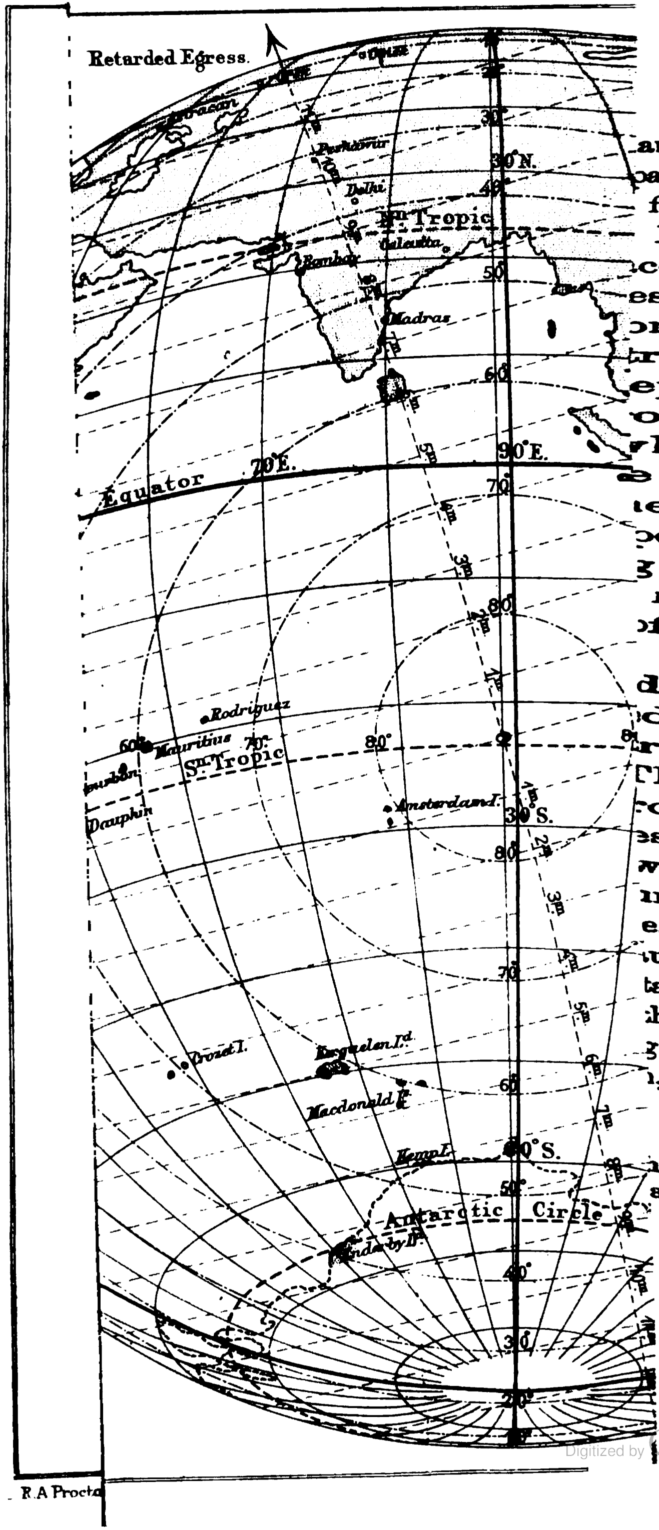

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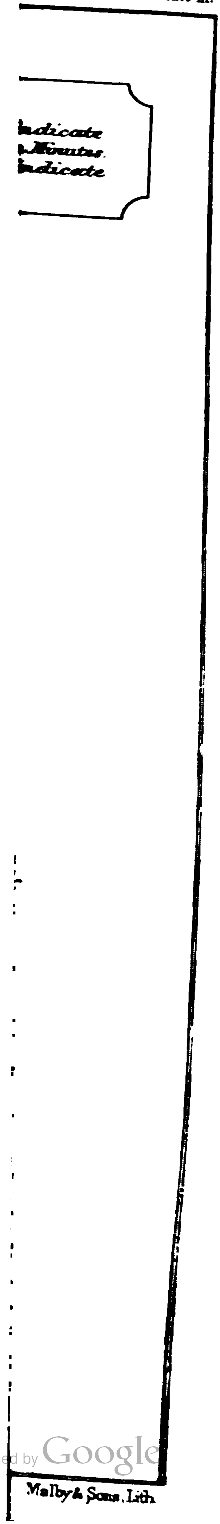
Plate IX.



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The six maps include four quarter-spheres, Plates VI. and VII., exhibiting the solar elevations and the coefficients of parallax.* The other two exhibit the Earth as supposed to be seen from the Sun at ingress and egress (*mean*, and for internal contacts). In these the solar elevations are indicated by circles, and in place of 10 parallactic parallels, corresponding to the parallactic circles in the other map, there are laid down parallactic lines corresponding to intervals of one minute (the line across the earth's centre being taken as a zero-line). These lines are not parallel, but separately constructed for. Thus their indications differ somewhat from those derived from the parallactic circles in the other maps, which are laid down on the supposition (not strictly correct) that the outline of the penumbra of Venus travels parallel to itself across the face of the earth. This will account for a slight want of correspondence between the second and third C-columns in the following tables: the third gives the correct effect of parallax. It will be noticed, however, that the difference is always trifling in the case of places suitable for the application of Delisle's method.

A further correction, but one of small importance, would result from the consideration that the apparent outline (supposed to be seen from the Sun) of that part of Venus's penumbra which traverses the Earth is not a straight line, but part of a large circle. Thus the actual outline on the Earth's surface is not part of a circle. It follows that the parallactic curves in the four quarter spheres ought not to be circular, and the parallactic lines across the other two maps ought to be curved, the direction of their curvature being turned in the direction towards which the shadow is moving for ingress, and the reverse for egress. All the corrections due to this cause are minute, and attain their greatest values at places not suitable as stations either for the application of Delisle's or Halley's method.

A correction has been applied to columns C corresponding to the fact that the maps are severally constructed for a single epoch, while the events to which they relate occupy several minutes.

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* See note on p. 384. The circles marked ·9, ·8, &c., pass through all the points at which ingress or egress is accelerated or retarded (as the case may be) by $\frac{9}{10}$ths, $\frac{8}{10}$ths, &c. of the maximum acceleration or retardation.

TABLE I.—*Accelerated Ingress.*

Station.	Sun's Elevation.			Coefficient of Parallax.		Acceleration in Minutes.	
	A	B	C	A	C	B	C
	°	°	°			m	m
Woahoo	22·5	...	19·8	·92	·93	...	11·2
Hawaii	22·3	21·5	19·7	·92	·92	10·3	11·1
Aiton I. (Aleutian)	12·0	...	10·8	·80	·84	...	10·3
Marquesas I.	20·0	23·1	17·7	·71	·66	7·5	7·9
Mouth of Amoor R.	15·0	...	14·0	·57	·62	...	7·6
Jeddo	...	30·9	32·1	...	·50	...	6·8
Otaheite	34·5	34·3	29·7	·59	·54	6·1	6·4
Nertchinsk	...	...	10·1	...	·41	...	5·8
Tsitsikar	...	...	17·0	...	·43	...	5·8
Kirin Oula	...	...	19·5	...	·42	...	5·7
Nagasaki	...	...	32·7	...	·40	...	5·3
Tientsin	...	...	22·2	...	·38	...	5·0
Pekin	...	20·2	20·8	...	·30	3·0	4·3
Shanghai	...	29·5	28·5	...	·25	2·6	3·9
Nankin	...	...	27·1	...	·20	...	3·6
Canton	...	...	35·5	...	·08	...	1·6
Hongkong	...	...	36·2	...	·08	...	1·6

TABLE II.—*Retarded Ingress.*

Station.	Sun's Elevation.			Coefficient of Parallax.		Retardation in Minutes.	
	A	B	C	A	C	B	C
	°	°	°			m	m
Crozet I.	9·5	...	15.0	·98	·96	...	12·6
Enderby Ld.	...	17·3	20·0	...	·92	10·3	11·8
Kerguelen Ld.	25·0	23·6	27·5	·91	·88	10·3	11·6
Macdonald I.	...	27·2	31·0	...	·85	...	11·2
Kemp I.	...	...	30·0	...	·85	...	11·1
Bourbon I.	4·5	...	12·4	·93	·84	...	11·1
Mauritius	6·0	...	14·1	·92	·81	...	10·7
Amsterdam I.	...	27·6	34·1	...	·77	9·8	10·3
Rodriguez	11·5	...	19·0	·89	·76	...	9·9
Sabrina Ld.	...	...	45·0	...	·73	...	8·2
Adelie Ld.	...	...	45·0	...	·50	...	6·8
S. Victoria Ld.	...	36·4	38·5	...	·46	5.0	6·0
Perth (Aust.)	...	...	65·0	...	·38	...	5·3
Royal Co. I.	...	...	62·0	...	·32	...	4·5
Madras	12·5	...	21·0	·47	·25	...	4·0
Bombay	4·5	...	12·5	·44	·22	...	3·8
Macquarie Ld.	...	...	52·0	...	·25	2·9	3·5
Hobart Town	...	70·0	67·0	...	·20	...	2·8
Adelaide	...	...	75·0	...	·18	...	2·5
Melbourne	...	...	75·0	...	·13	...	2·2

Table III.—*Accelerated Egress.*

Station.	Sun's Elevation.			Coefficient of Parallax.		Acceleration in Minutes.	
	A	B	C	A	C	B	C
	°	°	°			m	m
South Victoria Ld. (Possession I.)	...	23·8	25·0	...	·89	10·3	11·4
Adelie Ld.	...	...	34·0	...	·83	...	10·6
Campbell I.	...	...	26·0	...	·79	...	10·3
Emerald I.	...	...	30·0	...	·78	...	10·3
Macquarie I.	24·0	...	32·0	·83	·75	...	9·8
Chatham I.	11·5	...	16·0	·87	·75	...	9·8
Canterbury (N.Z.)	19·0	...	22·5	·81	·71	...	9·3
Wellington	17·0	...	20·0	·81	·70	...	9·2
Sabrina Ld.	...	...	43·0	...	·70	...	9·2
Enderby Ld.	...	41·0	39·0	...	·65	6·9	8·5
Royal Co. I.	...	...	42·0	...	·64	...	8·5
Auckland	15·0	14·4	19·2	·78	·64	8·6	8·5
Kemp I.	...	...	51·0	...	·57	...	7·6
Hobart Town	36·0	36·0	40·0	·66	·57	7·5	7·6
Melbourne	38·0	...	43 0	·58	·48	...	6·6
Sydney	33·0	...	37·2	·59	·48	...	6·6
Adelaide	...	...	47·8	...	·40	...	5·8
Kerguelen Ld.	...	60·5	57·1	...	·40	...	5·0
Crozet I.	...	...	47·5	...	·34	...	4·2
Perth (Aust.)	...	...	66·2	...	·25	...	3·6

Table IV.—*Retarded Egress.*

Station.	Sun's Elevation.			Coefficient of Parallax.		Retardation in Minutes	
	A	B	C	A	C	B	C
	°	°	°			m	m
Orsk	12·0	...	12·5	·98	·98	...	11·8
Omsk	12·5	...	11·5	·96	·97	...	11·7
Astracan	12·4	...	12·0	·97	·97	...	11·6
Aleppo	14·2	...	14·6	·91	·89	...	10·5
Peshawur	...	...	31·5	...	·85	...	10·3
Alexandria	13·0	...	14·0	·86	·84	...	10·0
Suez	16 0	17·6	16·1	·85	·83	9.4	9·8
Nertchinsk	...	...	10·1	...	·81	...	9·8
Delhi	...	...	38·0	...	·78	...	9·4
Tsitsikar	...	...	12·0	...	·72	...	8·7
Bombay	...	...	45·0	...	·70	...	8·5
Pekin	...	21·2	21·0	...	·70	7·7	8·6
Kirin-Oula	...	...	14·0	...	·69	...	8·4
Tientsin	...	...	17·1	...	·68	...	8·4
Calcutta	...	...	45·3	...	·68	...	8·2

Station.	Sun's Elevation.			Coefficient of Parallax.		Retardation in Minutes.	
	A	B	C	A	C	B	C
	°	°	°			m	m
Aden	...	...	36·0	...	·66	...	7·8
Nankin	...	...	27·0	...	·62	...	7·6
Madras	...	...	52·0	...	·61	...	7·4
Shanghai	...	26·1	26·0	...	·57	6·2	7·2
Canton	...	...	37·0	...	·52	...	6·6
Hongkong	...	...	37·0	...	·50	...	6·5

It will be seen, on a comparison of tables A, B, and C, that the effects of the change of phase are in some cases important. The coefficients of parallax are affected in several instances by more than 0·1, and in two cases by 0·22. In the cases of Crozet Island (Table II.) and Chatham Island (Table III.) solar elevations are so improved that these stations, which would have to be rejected if central passage were considered, are shown to be well suited for the observation of internal contacts. The diminution of all the coefficients in Table III., through the change of phase, has an important influence on the value of Delisle's method, so far as egress observations are concerned. It is important to notice, also, that under heads C in Tables III. and IV. many stations not hitherto recognised as available are included among the best places for observing egress. The Indian stations in Table IV. seem too valuable to be neglected. Peshawur is better even than Alexandria; Delhi is not inferior to the latter station (when solar elevation is considered as well as coefficient of parallax). Bombay, Calcutta, and Madras are also excellent. It may be noticed also that Bombay and Madras, which, when considered with reference to central passage, had seemed suitable places for the observation of retarded· ingress, are found to have so poor a coefficient of parallax when reference is made to internal contacts, that it would be useless to observe ingress there (so far at least as the application of Delisle's method is concerned).

Of course, it will be impracticable for this country to send observers to more than a certain number of stations. But it is not unlikely that besides Russia, France, and England (the only countries specially concerned in the transit of 1874), other nations may care to take part in the solution of the noble problem of determining the Sun's distance; and thus it seems advisable that all the stations where there will be any chance of obtaining useful observations, should be tabulated as nearly as possible according to their relative values.[*]

[*] It may not be amiss to point out that the numbers tabulated under head C in the above tables may be readily tested in a few minutes for any assigned station. Indeed, the whole work of determining the circumstances of the transit for internal contacts, as seen from any station, may be gone through in about an hour, and the main part of the work thus gone through would not require to be repeated for other stations. Although I have no reason to doubt the accuracy of the results tabulated above, it may be well that they should be checked. The justice of the following processes will be obvious to every one acquainted with the principles on which the transits of an inferior planet depend:—

Take out from the *Nautical Almanac* and tables of the planetary elements,—

App. Semi-diameter of Sun on December 8 ... = D
" " Venus in inf. conj. ... = V
Horizontal Parallax of Venus (in inf. conj.) refd. to Sun = P
Sun's Southern Declination on December 8 ... = δ
Equation of time ... ... = ϵ

On any convenient scale describe concentric circles with radii

(i.) D (ii.) D + V (iii.) D − V
(iv.) D − V + P (v.) D − V − P

On circle (ii.) take (according to Mr. Hind's elements of the transit) position-angles 130·7° from south towards east, and 160° from south towards west, and join the points thus determined. Determine with a protractor the position-angles Q_1, Q_2, Q_3, and Q_4, for the intersections of this chord with the circles (iv.) and (v.) Again, making the extremities of the chord correspond to hours $13^h\ 47^m$ and $18^h\ 26^m$ (from Mr. Hind's elements of the transit) divide the chord into hour-divisions. Thence determine epochs H_1, H_2, H_3, H_4, corresponding to the intersections of the chord with the circles (iv.) and (v.).

Now adjust a globe so that, looked at from above (vertically), its appearance shall be that presented by the Earth, supposed to be seen from the Sun, at apparent solar time $H_1 + \epsilon$, and with southerly solar declination δ. From the southern intersection of the brazen meridian with the horizon-circle, take an arc of Q_2 towards the east along the latter circle. This will give the place where ingress is most accelerated. To determine the value of any station near this point, measure the arc-distance (α) of the station from the point, and the distance (β) from the horizon-circle. Then $\cos \alpha$ = coefficient of parallax, and β = Sun's elevation. And thus the coefficients and solar parallax may be determined for any number of stations, with corresponding methods for retarded ingress and for egress.

It may be necessary to notice that, to convert coefficients of parallax into minutes of acceleration or retardation, reference must be made to the intervals of maximum acceleration and retardation. These are, for accelerated ingress and retarded egress (Tables I. and IV. respectively) $11^m\ 58^s$; for retarded

As regards the comparison between Delisle's and Halley's method for the transit of 1874, I may remark that M. Puiseux's results seem to have been somewhat misinterpreted. He does not anywhere speak of Halley's method as the best, but simply states that he can see no reason why it should not be applied: nor do his figures establish the superiority of Halley's method.

I believe I shall be able to show, however, that there is at least a possibility that Halley's method may be so applied to the transit of 1874 as to give absolutely the best means of determining the Sun's distance available before the transit of 2004 (about the circumstances of which I know nothing). It only requires that the same energy should be devoted (either by England or some other nation) to the coming transit, which has been called for in relation to the transit of 1882. There will also be the same chance of failure in one case as in the other.

To prove the justice of these views I point to Nertchinsk (or its neighbourhood) as a suitable station for observing the lengthened transit, and the neighbourhood of Enderby Land as a proper station for observing the shortened transit. The above tables give for the former station a lengthening of $(5{\cdot}7^{\mathrm{m}}+9{\cdot}8^{\mathrm{m}})$ or $15{\cdot}5^{\mathrm{m}}$; and for the latter a shortening of $(11{\cdot}8^{\mathrm{m}}+8{\cdot}5^{\mathrm{m}})$ or $20{\cdot}3^{\mathrm{m}}$. The total difference of duration is thus shown to be $35{\cdot}8^{\mathrm{m}}$. Setting against this the best cases for Delisle's method (Woahoo from Table I. and Crozet Island from Table II.), we get the total difference of absolute time, is $(11^{\mathrm{m}}{\cdot}2+12^{\mathrm{m}}{\cdot}6)$ or $23^{\mathrm{m}}{\cdot}8$. And the relative values of the methods in these cases (the most favourable for each) are given by the formula

$$\frac{\text{Halley's}}{\text{Delisle's}}=\frac{(35{\cdot}8)^2}{(23{\cdot}8)^2}\,\frac{\{(4{\cdot}28)^2+(1)^2\}^{*}}{(4{\cdot}28)^2}=\frac{2477^{\dagger}}{2075}\text{ approximately;}$$

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ingress and accelerated egress (Tables II. and III. respectively) $13^{\mathrm{m}}\ 8^{\mathrm{s}}$. A coefficient of parallax p corresponds for the former pair of cases to a time-interval

$$[11^{\mathrm{m}}\ 58^{\mathrm{s}}-35^{\mathrm{s}}\,(1-p]\,p,$$

for the latter to a time-interval

$$[13^{\mathrm{m}}\ 8^{\mathrm{s}}+35^{\mathrm{s}}\,(1-p)]\,p.$$

[*] Here I have used the values given in Mr. Stone's paper, *Monthly Notices*, April 8, pp. 251, 252. He would have obtained a larger value for e had he calculated for internal contacts. This would, of course, have been a change unfavourable to my case. But against this I set the fact that his careful examination of the circumstances of internal contacts will tend to render the probable errors much smaller (relatively) in 1874 than they were in 1769, when the observers had very vague notions of the phenomena they were to expect and to watch.

[†] It would not be correct to reduce the number of minutes 35·8 in a certain

and in this ratio, independently of all the other advantages which it presents, does Halley's method surpass Delisle's. By taking Kemp Island, Adelie Land, Victoria Land, Crozet Island, or Kerguelen's Land for the southern, and Tsitsikar, Kirin-Oula, or Tientsin, for the northern stations, we get differences of duration ranging from 33^{m} to $30{\cdot}6^{m}$, corresponding (according to Mr. Stone's formula) to a range from 24^{m} to $22{\cdot}2^{m}$, in the case of Delisle's method. There is little chance of the latter method being applicable with a difference greater than the lowest of these methods; and the highest is a difference which the best stations will not give by Delisle's method.

When it is remembered that Halley's method is so much the simpler, and that stations selected with reference to it give a double chance of at least a useful observation, the above considerations seem to decide the question of the relative values of the two methods in 1874.

Lastly, as to the relative values of the transits of 1874 and 1882, considered with reference to Halley's method.

Mr. Stone's remark, that the choice of stations is limited to those at which the Sun will have an elevation of at least 10°, reduces the maximum available difference of duration in 1874 from $36\frac{3}{4}^{m}$ (the value I had before mentioned) to $35\frac{3}{4}^{m}$. But the transit of 1882 is much more seriously affected. The suggested station near Sabrina Land must be rejected at once. And although the suggested station near South Victoria Land corresponds to an elevation of 10° at ingress, there is no accessible spot in that neighbourhood which will give any such elevation. At Possession Island the Sun's elevation will not be much more than 5° at ingress; and at Coulman Island, the most southerly station which antarctic seamen hope to reach, the Sun's elevation will be but 7°; and even if these islands were suitable, they give a difference of duration perceptibly less than that which I had dealt with in my former paper.

I must add that I had fully taken into account the difference in the clinging of the disc of Venus to the Sun's limb in 1874 and 1882. Indeed, I had adopted a considerably greater proportion than that indicated by Mr. Stone. But as the principle he lays down requires that both Sabrina Land and Victoria Land should be dismissed from our consideration in 1882—and as there is absolutely no other southern station at all comparable with these two, as far as

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proportion, and take the excess over $23^{m}{\cdot}8$, as measuring the superiority of Halley's mode. The proper way of treating the question is to indicate the relation between the two modes by a ratio, as above.

the lengthening of the transit's duration is concerned, we seem forced to the conclusion that if Halley's method fails totally for either of the coming transits, it is for that of 1882 and not for that of 1874.

On the general question of the value of the transit of 1874, I find myself compelled to adopt a different opinion from that which Mr. Stone has expressed. Although the comparative slowness of ingress and egress increases the probable error, it increases the available time differences in at least the same proportion—I say, *at least*, because I cannot believe that the errors of an observation of ingress or egress increase fully in the proportion of the slowness with which the phenomena succeed each other. But even assuming this unfavourable view, it may yet be shown that, *cæteris paribus*, slow transits (*i.e.* transits of short chord) are as valuable as more rapid ones when Halley's method is considered, and more valuable when Delisle's method is considered. For, in the first case, we have

(1), Value of a transit varies as maximum observable difference;

(2), Maximum observable difference varies as slowness of rate of ingress or egress * (*cæteris paribus*):

(3), Value of a transit varies as $\dfrac{1}{\text{Slowness of rate of ingress}}$;

therefore, for transits of different rates, but having circumstances otherwise equal, the value is appreciably constant, so long as the chord of transit is not too near a diameter of the Sun.

For the second case we have (1) and (2) unaltered, but in place of (3) we get the

$$\text{Relative value of transit varies as } \frac{1}{\left(\frac{k^2}{v^2}\right)+l^2};$$

where k is some constant, v represents the rate of ingress or egress, and l is the error in determining the longitude of a station. Thus, the value of a transit varies as $\frac{1}{v^2} \div \left(\frac{k^2}{v^2} + l^2\right)$ or inversely as $(k^2+v^2l^2)$, and the slower transits are therefore the most favourable.

It is worthy of notice, however, that, *as a rule*, when there are two transits separated by eight years, the first is the less favourable for the application of Halley's method. The present case shows that to this rule there are exceptions.

Monthly Notices of the Royal Astronomical Society for June 1869.

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* Estimated, of course, perpendicularly to the limb.

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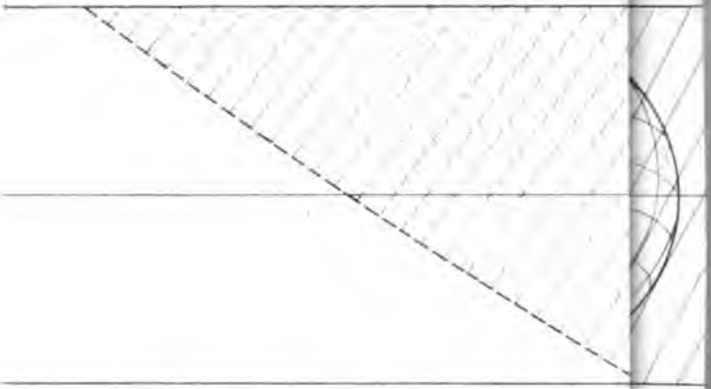

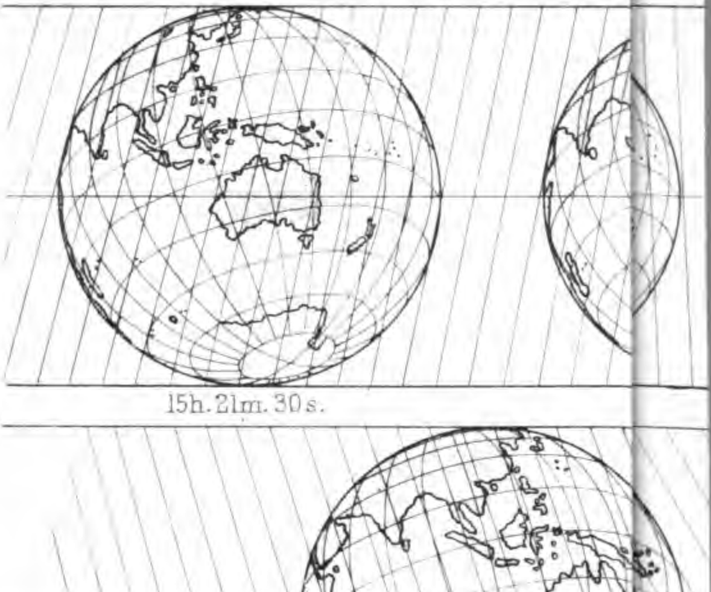

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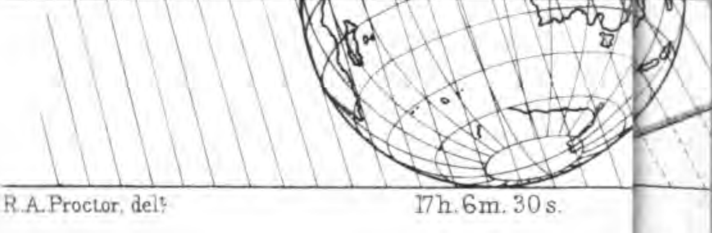

R.A.Proctor, del^t

17h. 6m. 30 s.

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ON THE APPLICATION OF PHOTOGRAPHY

AS A MEANS OF DETERMINING THE SOLAR PARALLAX FROM THE TRANSIT OF VENUS IN 1874.

It is impossible to read Mr. De la Rue's account of the results of careful measurement applied to photographs of the solar eclipses in 1860 and 1868 without recognising that we have in photography, as applied to the approaching Transit of Venus, one of the most powerful available means of determining the Sun's distance. Within the last few years, solar photography has made a progress which is very promising in regard to the future achievements of the science as an aid to exact astronomy. So that doubtless, in 1874, astronomers will apply photographic methods to the transit of that year, with even greater success than we should now be prepared to anticipate. It therefore seems to me that the photographic observation of the coming transit merits at least as full a preliminary inquiry as either Halley's or Delisle's method of direct observation.

The result of an inquiry directed to this end has led me to the conclusion that photographers of the approaching transit should adopt for their guidance considerations somewhat different from those which have hitherto been chiefly attended to.

It is undoubtedly true, as Mr. De la Rue has pointed out, that the photographer of the transit can readily take a large number of pictures, and by combining these, can ascertain with great accuracy the path of Venus across the solar disc. And by comparing the paths thus deduced for different stations a satisfactory estimate can be formed of the solar parallax I do not wish to suggest any departure from this course of procedure.

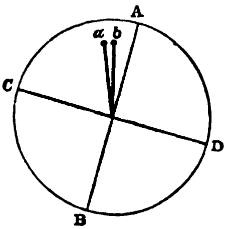
On the other hand, it is undoubtedly true, as Major Tennant has remarked, that the greatest effect of parallax will be obtained for any two stations, when both stations, the Earth's centre, and the centre of Venus, are in one and the same plane. So far as those two stations are concerned, his remark is just, that it is the position of Venus at the instant when the stations are so situated, and not the nearest approach of Venus to the Sun's centre, which should be compared. And further, Mr. De la Rue's comment on this, to the effect that his method in reality includes Major Tennant's, is also correct. In fact, there can be no doubt that the position of Venus at the particular instant referred to by Major Tennant can be far more exactly ascertained by a reference to the complete path of Venus for each station than from any attempt to secure nearly simultaneous photographic records at stations far removed from each other.

But it appears to me that the method I am about to suggest, according to which the whole question will be reduced to the determination of a parallactic displacement of Venus on a line through the centre of the Sun's disc, is the one by which the fullest assistance will be obtained from photography; while a source of error, which has not hitherto been specially considered, will be practically eliminated.

It must be remembered that in the comparison of photographic records, whether for the determination of the path of Venus across the Sun's disc at a particular station, or for the comparison either of Venus's apparent position or of her path as seen from two different stations, the accuracy of the results will depend in part on the certainty with which two or more pictures may be brought into comparison by means of a fiducial line or set of lines. It seems certain that no method can be devised by which all chance of error from this source can be eliminated. The great point would, therefore, seem to be to render its effect as small as possible.

Now let us consider for a moment Major Tennant's proposition, as giving a convenient illustration of the effects of any error either in the position of the fiducial lines, or in bringing those belonging to two pictures into exact correspondence. Let Fig. 21 represent the result of a comparison between two photographs of the Sun. A B and C D are fiducial cross-lines common to both pictures, *a* is the centre of Venus for one picture, *b* is her centre for the other; and on the exact measurement of *a b* depends the determination of the Sun's parallax, so far at least as these two pictures are concerned. Now it is very obvious that if the lines A B, C D, for one picture, have not been brought into perfect correspondence with those belonging to the

Fig. 21.

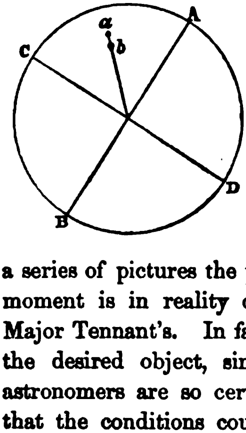


other, the distance *a b* will be correspondingly affected. In fact, it would appear that if the usual methods for making the correspondence as exact as possible are followed, almost as large an error would be introduced through this cause alone as by errors in the measurement of *a b*, since the two processes—the measurement of *a b* and the adjustment of the sets of cross-lines—depend on the very same circumstance, the nicety, namely, with which the eye and the judgment can estimate minute quantities of about the same relative dimensions.

But now, if *a* and *b*, in place of having the position shown in Fig. 21, were situated as in Fig. 22, it is clear that the distance *a b* will not be appreciably affected by any small error in the adjustment of the fiducial lines.

The object, therefore, which it seems most desirable to secure is that Venus, as seen from two different stations at a particular instant, should have a relative parallactic displacement towards the Sun's centre, or as nearly towards the Sun's centre as possible. This amounts to adding to Major Tennant's conditions this further one that the Sun's centre should be in the same plane with the two stations—or rather to making this condition a substitute for that one which requires that the Earth's centre should be in the same plane with the two stations. For as a rule we must not expect to be able to secure that two convenient stations on the Earth, as well as the centres of the Earth, Venus, and the Sun, should be in the same plane.

Fig. 22.



Mr. De la Rue's remark that by taking a series of pictures the position of Venus may be ascertained at any moment is in reality quite as applicable to my suggestion as to Major Tennant's. In fact, were it not, we might despair of securing the desired object, since we have no reason for believing that astronomers are so certain as to the exact progress of the transit, that the conditions could be secured by anticipatory instructions: whereas by applying Mr. De la Rue's method it will be possible after the transit is past, to determine with any desired degree of accuracy the position of Venus at the proper instant. And further, it is very obvious that no error in the placing of the fiducial lines for pictures taken at the same stations, can much affect the accuracy of the result, since the comparison of successive pictures taken at the same station does not directly involve the element of the solar parallax, as when we have to compare pictures obtained at different stations.

The object, then, of the present paper and the accompanying chart is to determine what stations are most suitable for applying photography to the transit of 1874, on the principles above enumerated. I think the drawing will be found, however, to be also an instructive illustration of the whole character of the transit.

In the preceding paper in the 'Monthly Notices' for March I showed how all the chief elements of the transit could be deduced by considering the motion of Venus relatively to a pair of cones, each enveloping the Sun and the Earth, but one having its vertex outside the Earth, the other having its vertex between the Earth and the Sun. For my present purpose it will be convenient to consider the motion of the Earth relatively to a pair of cones similarly enveloping the Sun and Venus.

FIG. 23.

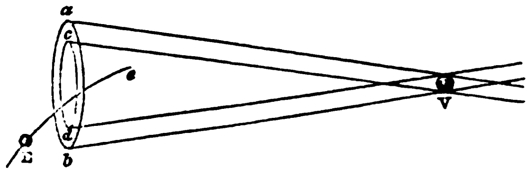
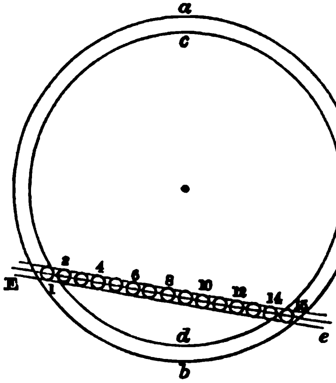


Fig. 23 represents a portion of these cones around Venus at V. E *e* is the Earth's orbit relatively to these cones; *a b* is the circular section of the outer cone, *c d* that of the inner. Venus is supposed to be approaching the eye, and therefore the Earth also would, at the time of conjunction, be approaching; but as we are considering the Earth's motion relatively to the two cones, and as Venus moves more rapidly in her orbit than the Earth, we must suppose the Earth to traverse the sections *a b* and *c d* in direction E *e*. When the circumstances of the transit of 1874 are attended to, it results that the motion of the Earth relatively to the sections *a b* and *c d* is as shown in Fig. 24. The various circles represented along the parallels E *e* correspond to the various positions of the Earth represented in the illustrative plate.

Comparing Figs. 23 and 24, it will be seen at once that, as soon as the Earth reaches the outer circle *a b*, external contact begins. With the peculiarities of this phase we need not concern ourselves, When the Earth reaches the circle *c d*, as at position 1, Fig. 24, internal contact begins, and when the Earth just touches the circle *c d* on the inside, the transit has begun for all places on the Earth's illuminated hemisphere. The positions 1 and 2 correspond to the cases of internal contact most accelerated and internal contact most retarded. They have been added to the illustrative plate for the sake of completeness, but in reality they do not belong to the special subject of this paper since, as Mr. De la Rue has remarked, the photographer need not set himself to observe special phases of this sort. The same remark applies to the positions 14 and 15.

Fig. 24.



The remaining positions of the Earth in Fig. 24, corresponding to the 11 pictures 3–13 in Plate X., are those occupied by the Earth at successive intervals of 15 minutes, the picture numbered 8 corresponding to the position occupied by the Earth at $16^h\ 6^m\ 31^s$ G.M.T., on December 28, 1874, when Venus makes her nearest approach to the centre of the Sun's disc.

Now if we look at Figs. 23 and 24, and consider what they represent, we shall see that Fig. 24 may be looked upon as exhibiting an inverted picture of the Sun's disc and the transit of Venus's centre across it: we see, in fact, that the apparent position occupied at any instant by any point on the Earth's surface in Fig. 24, corresponds exactly to the position occupied by Venus upon the Sun's disc, as supposed to be seen from that point of the Earth's surface at the instant in question. We have only to invert Fig. 24, and look at it from behind to see what sort of path Venus would seem to traverse upon the Sun's disc, either with reference to the Earth's centre, or to any point of the Earth's surface supposed to be properly depicted upon the small figures 1–15.

It follows, therefore, that if we want to determine two stations at which at any instant Venus would appear to have a relative parallactic displacement towards the Sun's centre, all that is required is that we select two stations which are on the same radial line from the common centre of the circular sections *a b* and *c d*.

The positions of those radial lines which cross the Earth's track through the section *c d* are exhibited in Plate X. It will be understood, of course, that the three rows of figures belong in reality to a single row, the numbering of the successive pictures of the Earth indicating the way in which that row would be formed by the combination of the three rows shown in the plate.

I need not explain the construction of the plate, which depends on the simplest mathematical principles. I have taken a considerable amount of care to secure accuracy, not only in the projections of the Earth, but in the position of the radial cross-lines; and, though there may be minute inexactnesses, there will be found none, I think, which affect the purpose for which the plate was constructed. What that purpose is will be best illustrated by simply examining the indications of the successive pictures.

Passing over pictures 1 and 2, we notice in Fig. 23, that Kerguelen's Land and Crozet Island, lying nearly on a line with certain of the Aleutian Islands, suggest that pictures taken at the former stations at the beginning of the transit could be advantageously compared with pictures simultaneously, or almost simultaneously, taken at a station on one of the easternmost of the Aleutians. In like manner pictures taken near Enderby Land could be advantageously compared with pictures taken at Woahoo. Projection 4 does not differ much from the preceding, but the cross-lines have assumed a less inclined position, and Kerguelen's Land could, at the epoch belonging to this picture, be better combined with a somewhat more westerly Aleutian island.

Projection 5 exhibits the advantage of a photographic station at or near Yokohama. Probably such a station, combined with one in Crozet Island or Kerguelen's Land, would give (by pictures taken near the hour belonging to Projection 5) absolutely the best results which photography can give.

The remaining projections suggest the following combinations of photographic records:—

Projection 6. Yokohama and Enderby Land, Kerguelen's Land and a station in Manchooria; Crozet Island and Pekin; Cape of Good Hope and Nertchinsk.

Projection 7. Kerguelen's Land and Tsitsikar; Crozet Island and Nertchinsk; Cape Town and a station west of Lake Baikal.

Projection 8. Kerguelen's Land and Nertchinsk; Cape Town and Peshawur; Repulse Bay or neighbourhood and Yokohama; Perth (Australia) and Yokohama.

Projection 9. Repulse Bay and Yokohama; Enderby Land and Nertchinsk; Crozet Island and Calcutta; Cape Town and Bombay.

Projection 10. Repulse Bay and Nertchinsk; Possession Island (near South Victoria Land) and Yokohama; Kerguelen's Land and Calcutta; Crozet Island and Peshawur; Cape Town and Teheran.

Projection 11. Possession Island and Tsitsikar; Repulse Bay and neighbourhood of Lake Baikal; Enderby Land and Calcutta; Kerguelen's Land and Madras; Crozet Island and Peshawur; Cape Town and Aden.

Projection 12. Possession Island and Nertchinsk; Enderby Land and Madras; Kerguelen's Land and Peshawur; Crozet Island and Teheran.

Projection 13. Possession Island and neighbourhood of Lake Baikal; Repulse Bay and Calcutta; a New Zealand station and Yokohama; Hobart Town and a station near the mouth of the Amoor.

From this list we see that Kerguelen's Land and Crozet Island, Peshawur* and other Indian stations, and stations in Siberia, are those which give the most favourable opportunities for the application of the photographic method.

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* Peshawur is in many respects unsuitable as an observing station. But Delhi would serve admirably.

Monthly Notices of the Royal Astronomical Society for January 1870.

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